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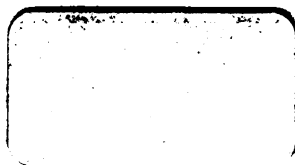
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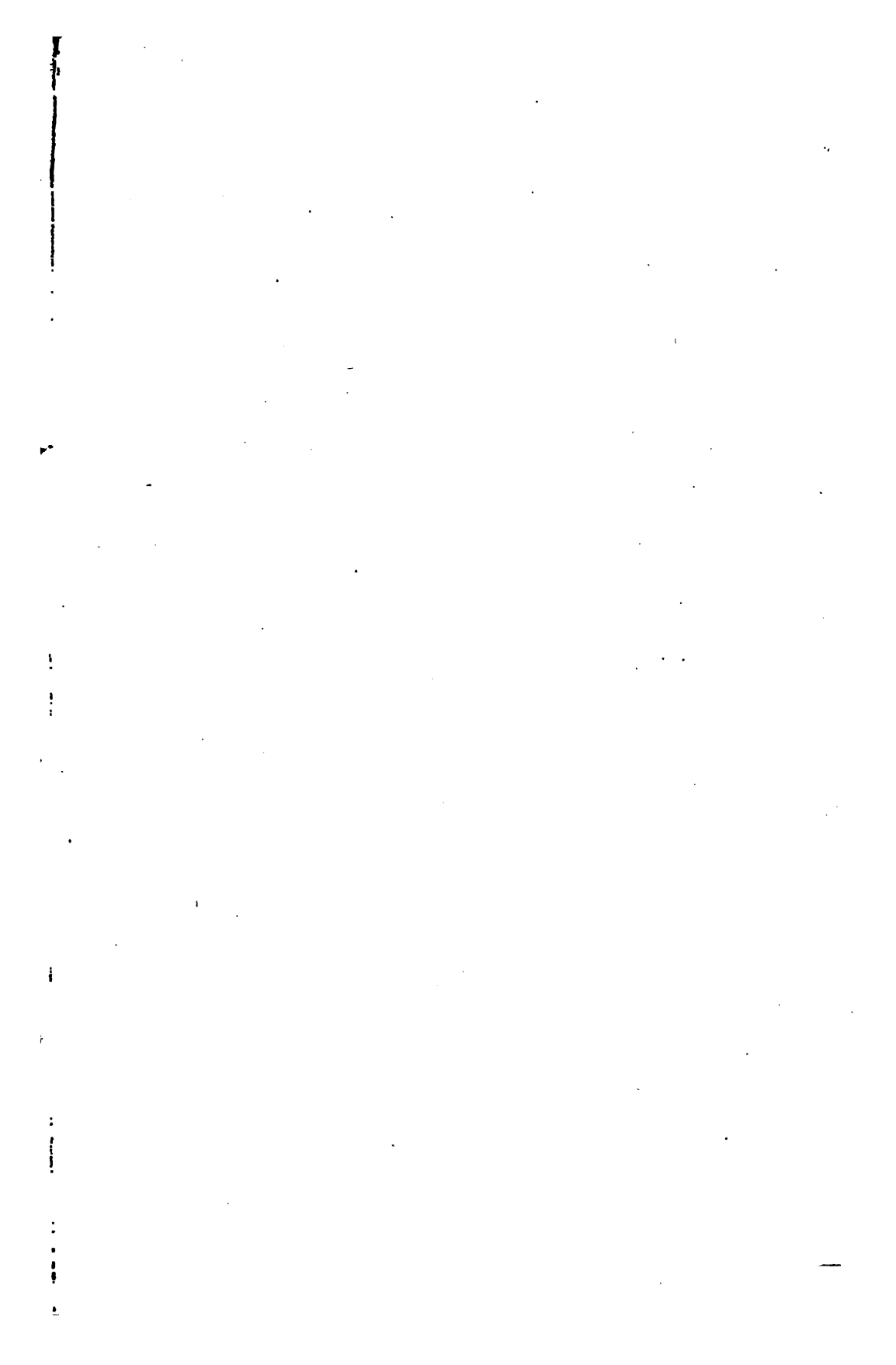
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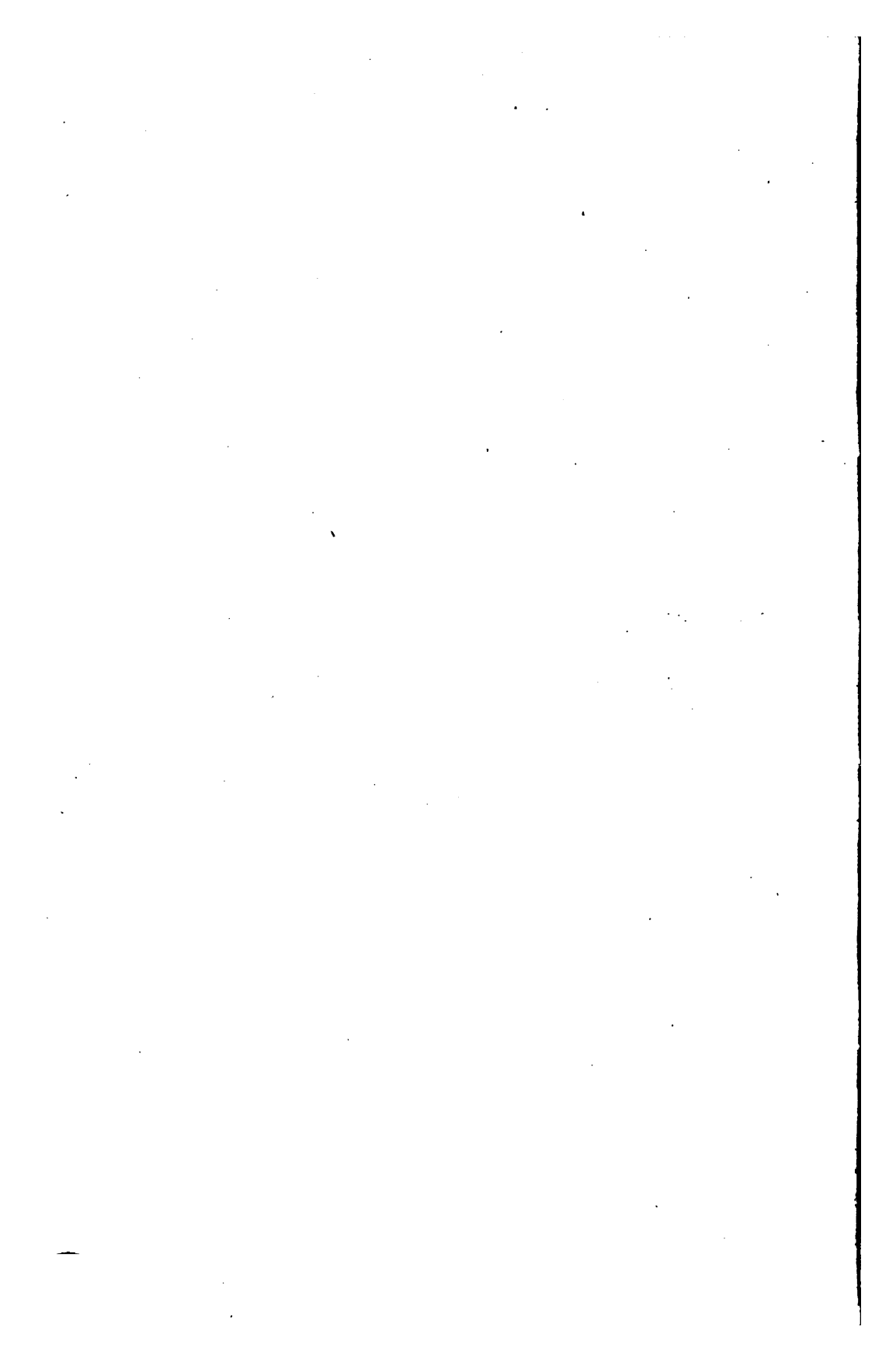


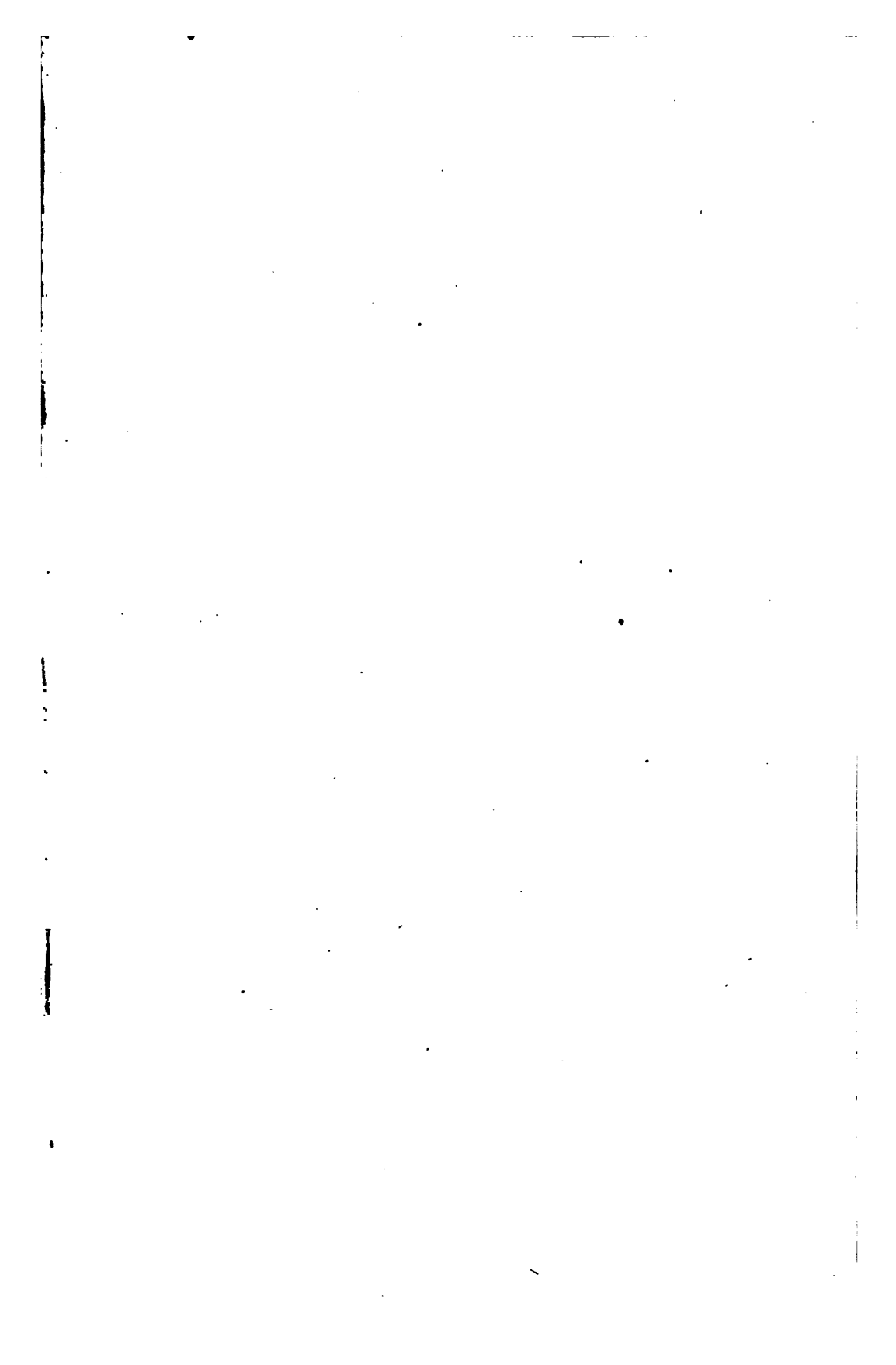
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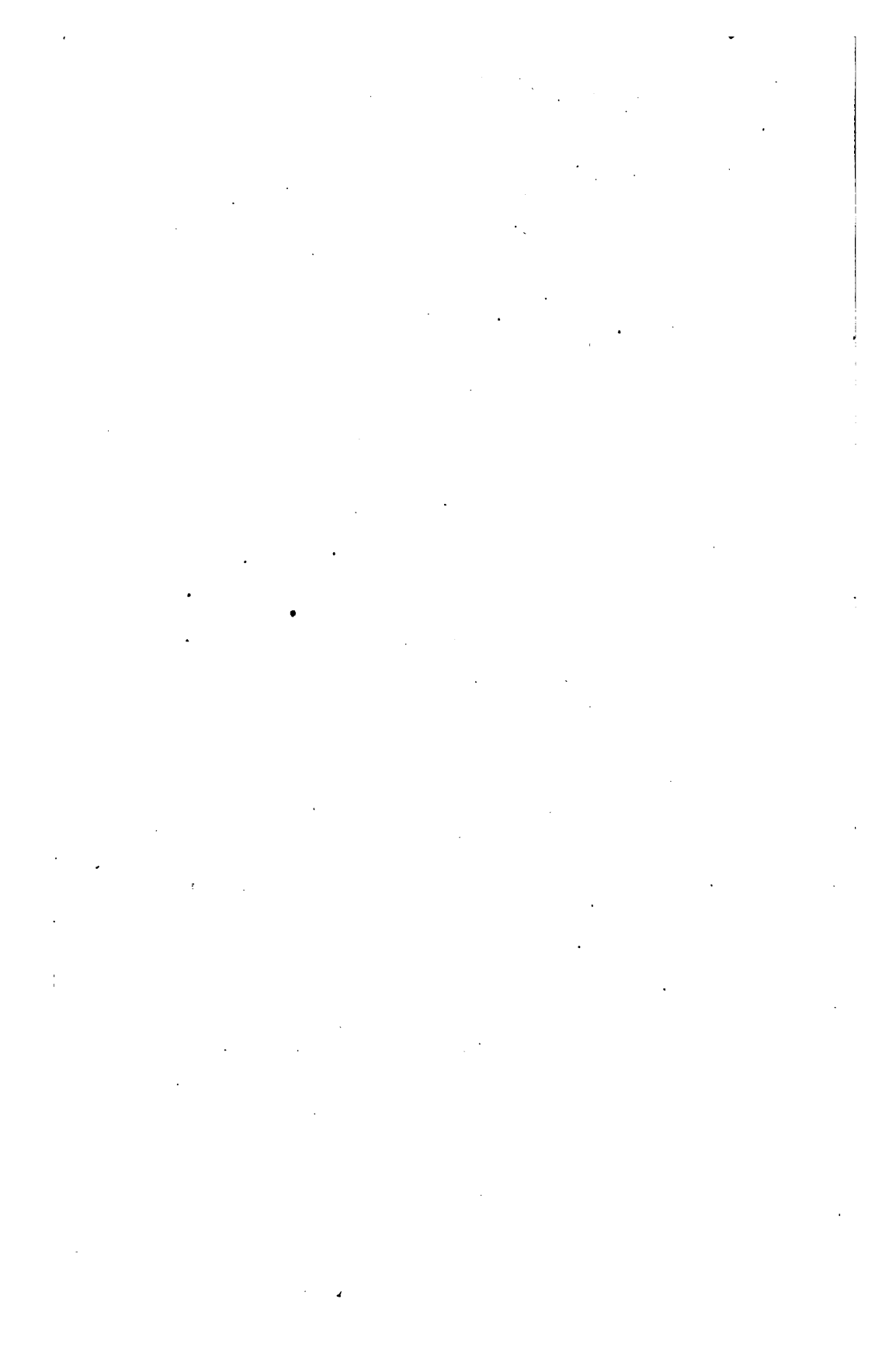
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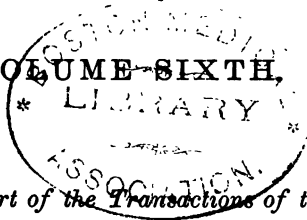








PROCEEDINGS
OF THE
PHILADELPHIA COUNTY
MEDICAL SOCIETY.

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VOLUME SIXTH,
LIBRARY
ASSOCIATION.

*Containing the Report of the Transactions of the Society from
September, 1883, to June, 1884.*

EDITED BY
THE PUBLICATION COMMITTEE.

PHILADELPHIA:

1884.

WM. P. KILDARE, PRINTER, 734 & 736 SANSON ST.

P R E F A C E.

THE present volume is the first printed by the Society entirely under its own charge, and a comparison of it with previous volumes will show how far this plan has resulted in increasing the value of the proceedings.

The contents of the volume are doubtless fully equal to those of earlier issues; the system of discussion, as originated by the President, Dr. Welch, still maintains its superiority over the methods of former years. It is noticeable that over one-fourth the work is occupied by communications on the nature of tuberculosis, the different articles representing all phases of the controversy.

The loss in membership by death has been unusually large, double that of previous years, and includes two of the most distinguished members: Dr. Samuel D. Gross, who has been a conspicuous figure in American surgery for a quarter of a century, and Dr. Thos. S. Kirkbride, whose name is indissolubly connected with alienology.

The Society does not necessarily endorse the opinions or statements of the authors.

WILLIAM R. D. BLACKWOOD.

EDWARD J. NOLAN.

HENRY LEFFMANN.

September 1, 1884.

FORMER PRESIDENTS.

SAMUEL JACKSON, M.D., elected 1849, '50, '51, '52.
JOHN F. LAMB, M.D., elected 1853.
THOMAS F. BETTON, M.D., elected 1854.
D. FRANCIS CONDIE, M.D., elected 1855.
WILSON JEWELL, M.D., elected 1856.
GOUVERNEUR EMERSON, M.D., elected 1859.
JOHN BELL, M.D., elected 1858.
BENJAMIN H. COATES, M.D., elected 1859.
ISAAC REMINGTON, M.D., elected 1860.
JOSEPH CARSON, M.D., elected 1861.
ALFRED STILLÉ, M.D., elected 1862.
SAMUEL D. GROSS, M.D., elected 1863.
LEWIS P. GEBHARD, M.D., elected 1864.
NATHAN L. HATFIELD, M.D., elected 1865.
WILLIAM MAYBURY, M.D., elected 1866.
ANDREW NEBINGER, M.D., elected 1867.
GEORGE HAMILTON, M.D., elected 1868.
WILLIAM L. KNIGHT, M.D., elected 1869.
WILLIAM H. PANCOAST, M.D., elected 1870.
JAMES AITKEN MEIGS, M.D., elected 1871.
D. HAYES AGNEW, M.D., elected 1872.
WILLIAM B. ATKINSON, M.D., elected 1873.
WASHINGTON L. ATLEE, M.D., elected 1874.
WILLIAM GOODELL, M.D., elected 1875.
THOMAS M. DRYSDALE, M.D., elected 1876.
HENRY H. SMITH, M.D., elected 1877, '78, '79.
ALBERT H. SMITH, M.D., elected 1880, '81.
HORACE Y. EVANS, M.D., elected 1882.
WILLIAM M. WELCH, M.D., elected 1883, '84.

OFFICERS AND COMMITTEES.

1884.

President.

WILLIAM M. WELCH.

Vice-Presidents.

WILLIAM S. FORBES.

S. R. KNIGHT.

Recording and Reporting Secretary.

HENRY LEFFMANN.

Corresponding Secretary.

M. S. FRENCH.

Assistant Secretary.

WM. C. HOLLOPETER.

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LOUIS K. BALDWIN.

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Censors.

WM. T. TAYLOR, one year.

CHAS. K. MILLS, three years.

F. P. HENRY, two years.

N. L. HATFIELD, four years.

H. ST. CLAIR ASH, five years.

Directors.

J. T. ESKRIDGE, Ch.

L. K. BALDWIN.

A. H. SMITH.

W. W. KEEN.

CHAS. T. HUNTER.*

Meteorology and Epidemics.

J. HOWARD TAYLOR, Ch.

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L. TURNBULL.

J. G. RICHARDSON.

BENJAMIN LEE.

Clinical Pathology.

F. P. HENRY, Ch.

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CHAS. M. SELTZER.

J. C. WILSON.

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CHAS. W. DULLES.

E. T. BRUEN.

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CARL SEILER.

C. H. BURNETT.

J. B. ROBERTS.

JAS. TYSON.

J. SOLIS-COHEN.

M. O'HARA.

J. T. ESKRIDGE.

WM. S. LITTLE.

JNO. G. LEE.

CHAS. K. MILLS.

H. F. FORMAD.

R. J. LEVIS.

Gynecology and Obstetrics.

B. F. BAER, Ch.

WM. H. PARISH.

WM. B. ATKINSON.

WM. T. TAYLOR.

HENRY BEATES.

Publication.

W. R. D. BLACKWOOD, Ch.

E. J. NOLAN.

HENRY LEFFMANN.

Hygiene, and the Relations of the Profession to the Public.

R. A. CLEEMANN, Ch.

WM. S. JANNEY.

JNO. H. PACKARD.

H. H. SMITH.

GEO. HAMILTON.

J. F. STONE.

H. Y. EVANS.

F. B. HAZEL.

J. F. HOLT.

* Deceased.

LIST OF MEMBERS.

WITH DATES OF GRADUATION IN MEDICINE, AND ELECTION TO
MEMBERSHIP IN THE SOCIETY.

Elected.

1883 (October). AUSTIN FLINT, SR., 418 Fifth Avenue, New York City. Harvard University, Medical Department, 1833.

HONORARY.

ACTIVE.

Elected.

1880 (June). GRIFFITH E. ABBOT, 106 Indian Queen Lane. University of Pennsylvania, 1879.
1867 (January). JOHN M. ADLER, 1028 Arch Street. National Medical College, Georgetown, D. C., 1861.
1858 (April). D. HAYES AGNEW, 1611 Chestnut Street. University of Pennsylvania, 1838.
1882 (January). JOHN J. ALEXANDER, 1236 North Seventeenth Street. University of Pennsylvania, 1880.
1875 (January). HARRISON ALLEN, 117 South Twentieth Street. University of Pennsylvania, 1861.
1875 (July). JOSHUA G. ALLEN, 1237 Spruce Street. University of Pennsylvania, 1856.
1874 (Jan.). OSCAR HUNTINGDON ALLIS, 1604 Spruce Street. Jefferson Medical College, 1866.
1883 (October). EDWARD W. ALLISON, 815 Spruce Street. University of Pennsylvania, 1880.
1881 (January). J. M. ANDERS, 1529 North Eighth Street. University of Pennsylvania, 1877.
1869 (January). THOMAS HOLLINGSWORTH ANDREWS, 1117 Spruce Street. Jefferson Medical College, 1864.
1884 (January). WM. M. ANGNEY, 519 Spruce Street. Jefferson Medical College, 1878.
1855 (July). HENRY ST. CLAIR ASH, 1112 Vine Street. Pennsylvania Medical College, 1850.
1880 (June). JOHN ASHHURST, JR., 2000 West Delancey Place. University of Pennsylvania, 1860.
1868 (July). SAMUEL ASHHURST, 2308 West Delancey Place. University of Pennsylvania, 1861.

Elected.

1849 (January). WILLIAM ASHMEAD, 4788 Germantown Avenue. University of Pennsylvania, 1826.
1860 (October). SAMUEL K. ASHTON, School House Lane; Office, 222 South Eighth Street. University of Pennsylvania, 1843.
1883 (April). WM. EASTERLY ASHTON, 222 South Eighth Street. University of Pennsylvania, 1881.
1864 (July). WILLIAM BIDDLE ATKINSON, 1400 Pine Street. Jefferson Medical College, 1858.
1883 (October). L. W. ATLEE, 210 South Thirteenth Street. Jefferson Medical College, 1882.
1883 (April). WALTER F. ATLEE, 210 S. Thirteenth Street. University of Pennsylvania, 1850.
1883 (October). JOHN AULDE, 4719 Frankford Avenue. Jefferson Medical College, 1882.
1879 (April). B. F. BAER, 2004 Chestnut Street. University of Pennsylvania, 1876.
1877 (April). WASHINGTON HOPKINS BAKER, 1610 Summer Street. University of Pennsylvania, 1875.
1870 (July). LOUIS K. BALDWIN, 1900 Wallace Street. Jefferson Medical College, 1862.
1869 (January). DAVID MILLER BARR, 1902 Spring Garden Street. Jefferson Medical College, 1864.
1881 (January). ROBERTS BARTHOLOW, 1509 Walnut Street. University of Maryland, 1862.
1878 (June). ISAAC BARTON, 128 South Fifteenth Street. Jefferson Medical College, 1877.

Elected.

- 1871 (January). J. MORRIS BARTON, 1344 Spruce Street. Jefferson Medical College, 1868.
- 1883 (October). CHARLES BAUM, 630 N. Broad Street. University of Pennsylvania, 1877.
- 1868 (July). HENRY FLICKWIR BAXTER, 242 Catharine Street. University of Pennsylvania, 1864.
- 1880 (June). HENRY BEATES, JR., 445 North Seventh Street. University of Pennsylvania, 1879.
- 1881 (January). EDWARD H. BELL, 322 South Fifth Street. Jefferson Medical College, 1875.
- 1878 (October). J. R. F. BELL, 1900 North Eleventh Street. University of Pennsylvania, 1869.
- 1860 (January). HENRY D. BENNER, 841 South Third Street. University of Pennsylvania, 1854.
- 1878 (January). WILLIAM H. BENNETT, 332 South Fifteenth Street. University of Pennsylvania, 1869.
- 1876 (January). EUGENE PROSPER BERNARDY, 221 South Seventeenth Street. University of Pennsylvania, 1868.
- 1878 (January). J. B. W. BIDLACK, 1335 Arch Street. University of Pennsylvania, 1869.
- 1882 (June). H. S. BISSEY, 1601 Columbia Avenue. University of Pennsylvania, 1880.
- 1873 (July). WILLIAM R. D. BLACKWOOD, 246 North Twentieth Street. University of Pennsylvania, 1862.
- 1884 (January). THOS. T. BLAND, 1622 Christian Street. Bellevue Medical College Hospital, 1874.
- 1881 (October). MAX H. BOCHROCH, 1210 North Seventh St. Jefferson Medical College, 1880.
- 1863 (July). CHARLES S. BOKER, 1622 Chestnut Street. University of Pennsylvania, 1852.
- 1851 (January). AUGUSTUS C. BOURNONVILLE, 516 North Sixth Street. Jefferson Medical College, 1847.
- 1883 (January). A. H. BOYER, 3260 Richmond Street. University of Pennsylvania, 1868.
- 1873 (June). T. H. BRADFORD, 1905 Pine Street. Jefferson Medical College, 1874.
- 1884 (June). CONRAD R. BREADY, 1921 North Seventh St. Jefferson Medical College, 1880.
- 1870 (October). DANIEL GARRISON BRINTON, 115 South Seventh Street. Jefferson Medical College, 1860.
- 1878 (June). J. H. BRINTON, 1423 Spruce Street. Jefferson Medical College, 1852.
- 1884 (June). LEWIS BRINTON, 1701 Race Street. Jefferson Medical College, 1882.
- 1883 (January). ALEXANDER BROWN, 630 North Sixteenth Street. University of Pennsylvania, 1867.

Elected.

- 1877 (July). EDWARD TUNIS BEUEN, 1814 Locust Street and 1631 Chestnut Street. University of Pennsylvania, 1873.
- 1882 (January). J. E. BRUNET, 1750 North Thirteenth Street. Jefferson Medical College, 1873.
- 1868 (January). FREDERICK J. BUCK, 770 South Fifteenth Street. University of Pennsylvania, 1856.
- 1877 (October). WILLIAM PENN BUCK, 2010 Arch Street. University of Pennsylvania, 1869.
- 1873 (January). WILSON BUCKBY, 1652 North Tenth St. Jefferson Medical College, 1870.
- 1881 (January). ROSS R. BUNTING, Ridge and Roxborough Avenues. Jefferson Medical College, 1856.
- 1876 (January). CHARLES H. BURNETT, 127 South Eighteenth Street. University of Pennsylvania, 1867.
- 1873 (April). R. BRUCE BURNS, 4325 Frankford Avenue. University of Pennsylvania, 1871.
- 1875 (July). WILLIAM AUGUSTUS BURNS, 1216 Spring Garden Street. University of Pennsylvania, 1869.
- 1879 (October). CHARLES E. CADWALLADER, 240 South Fourth St. University of Pennsylvania, 1861.
- 1884 (April). D. W. CADWALLADER, 110 South Thirteenth Street. University of Pennsylvania, 1880.
- 1876 (January). ALEXANDER CALDWELL, 1904 Christian St. Jefferson Medical College, 1869.
- 1882 (January). J. MOORE CAMPBELL, 1334 South Tenth Street. Jefferson Medical College, 1878.
- 1878 (January). J. A. CARNROSS, 1426 Walnut Street. Jefferson Medical College, 1876.
- 1882 (October). FREDERIC CARRIER, 40 North Sixteenth Street. Jefferson Medical College, 1878.
- 1869 (January). WILLIAM CARROLL, 617 South Sixteenth Street. Jefferson Medical College, 1863.
- 1875 (April). FRANKLIN DICK CASTLE, 419 South Fifteenth Street. University of Wurtzburg, Bavaria, 1870.
- 1877 (April). A. F. CHASE, 624 North Fortieth Street. Jefferson Medical College, 1874.
- 1872 (October). JOHN H. W. CHESTNUT, 1757 Frankford Avenue. University of Pennsylvania, 1871.
- 1882 (June). G. MAXWELL CHRISTINE, 1105 Diamond Street. University of Pennsylvania, 1880.
- 1872 (Jan.). LEONARDO STREET CLARK, 1505 Girard Avenue. University of Pennsylvania, 1867.
- 1872 (January). RICHARD A. CLEEMANN, 2135 Spruce Street. University of Pennsylvania, 1862.
- 1875 (April). J. SOLIS COHEN, 1431 Walnut Street. University of Pennsylvania, 1860.

Elected.

- 1882 (October). MORRIS S. COHEN, Jewish Hospital. Jefferson Medical College, 1881.
- 1884 (June). SOLOMON SOLIS-COHEN, 1229 Franklin Street. Jefferson Medical College, 1883.
- 1887 (January). JAMES COLLINS, 536 Marshall Street. University of Pennsylvania, 1880.
- 1882 (January). DENNIS N. CONNER, 1506 Girard Avenue. University of Pennsylvania, 1887.
- 1886 (October). JOSIAH C. COOPER, 1103 Arch Street. Jefferson Medical College, 1882.
- 1878 (April). T. V. CRANDALL, 1928 Spring Garden Street. College of Physicians and Surgeons, New York, 1886.
- 1888 (April). T. STANTON CROWLY, 2040 Locust Street. Jefferson Medical College, 1849.
- 1878 (January). R. B. CRUIER, 731 North Seventeenth Street. University of Pennsylvania, 1889.
- 1877 (April). WM. R. CRUIER, 2336 North Sixth Street. University of Pennsylvania, 1886.
- 1881 (April). JAMES CUMMISKEY, 2026 Vine Street. Jefferson Medical College, 1856.
- 1882 (June). CHARLES A. CURRIE, 5118 Germantown Ave. University of Pennsylvania, 1880.
- 1878 (June). ROLAND G. CURTIN, 22 So. Eighteenth Street. University of Pennsylvania, 1886.
- 1860 (October). LEVI CURTIS, 458 North Sixth Street. Jefferson Medical College, 1847.
- 1879 (October). J. C. DA COSTA, 1633 Arch Street. Jefferson Medical College, 1878.
- 1861 (April). JAMES DARRACH, Green St. near Rittenhouse, Germantown. University of Pennsylvania, 1861.
- 1878 (April). D. DAVIDSON, 902 Walnut Street. University of Pennsylvania, 1871.
- 1883 (October). GWILYM G. DAVIS, 1817 Mt. Vernon Street. University of Pennsylvania, 1879.
- 1880 (October). A. C. DEAKYNE, 832 Pine Street. Pennsylvania Medical College, 1858.
- 1881 (October). JOHN B. DEAYER, 1610 Vine Street. University of Pennsylvania, 1878.
- 1883 (April). RICHARD W. DEAYER, 6075 Main Street, Germantown. University of Pennsylvania, 1874.
- 1883 (January). FRANCIS X. DERGUM, 636 North Eighth Street. University of Pennsylvania, 1877.
- 1882 (October). W. C. DIXON, 4039 Baltimore Avenue. University of Pennsylvania, 1860.
- 1880 (October). JOHN F. DONNELLY, 1216 Christian Street. Jefferson Medical College, 1866.

Elected.

- 1863 (April). THOMAS MURRAY DRYSDALE, 1631 Arch Street. Pennsylvania Medical College, 1862.
- 1877 (July). EDWARD LOUIS DUER, 1704 Arch Street. University of Pennsylvania, 1860.
- 1870 (January). LOUIS A. DUHRING, 1411 Spruce Street. University of Pennsylvania, 1867.
- 1878 (April). CHARLES W. DULLES, 3962 Locust Street. University of Pennsylvania, 1875.
- 1882 (June). A. J. DUNDORN, 1400 North Twenty-third Street. Jefferson Medical College, 1866.
- 1863 (January). RICHARD JAMES DUNGLISON, 814 North Sixteenth Street. Jefferson Medical College, 1866.
- 1871 (July). GEORGE BENSON DUNMIRE, 1116 Arch Street. Jefferson Medical College, 1865.
- 1881 (June). HENRY E. DWIGHT, 336 South Fifteenth Street. University of Pennsylvania, 1867.
- 1883 (June). JOS. F. EDWARDS, 115 South Seventh Street. University of Pennsylvania, 1874.
- 1884 (April). W. A. EDWARDS, 1210 Spruce Street. University of Pennsylvania, 1881.
- 1884 (June). F. H. ELDER, 1437 Walnut Street. University of Pennsylvania, 1881.
- 1866 (January). ISAAC S. ESHLEMAN, 1738 Girard Avenue. Jefferson Medical College, 1861.
- 1879 (April). J. T. ESKRIDGE, 1622 North Sixteenth Street. Jefferson Medical College, 1876.
- 1876 (January). EDWIN LEWIS EVANS, 1700 Pine Street. University of Pennsylvania, 1870.
- 1858 (April). HORACE EVANS, 635 Walnut Street. University of Pennsylvania, 1831.
- 1864 (July). HORACE Y. EVANS, 1631 Green Street. University of Pennsylvania, 1868.
- 1884 (April). H. E. EVERETT, 238 North Twelfth Street. Jefferson Medical College, 1882.
- 1883 (June). G. GRANVILLE FAUGHT, 142 North Fifteenth Street. University of Pennsylvania, 1879.
- 1876 (April). ADOLPH FELDSTEIN, 868 North Sixth Street. University of Prague, 1864.
- 1879 (April). THOMAS H. FENTON, 1836 Arch Street. University of Pennsylvania, 1877.
- 1884 (January). WM. N. FERGUSON, 116 W. York Street. University of Pennsylvania, 1879.
- 1868 (July). EMIL FISCHER, 727 North Sixth St. Jefferson Medical College, 1855.
- 1884 (January). FRANK FISHER, 2015 Cherry Street. Jefferson Medical College, 1875.

Elected.

- 1881 (April). LAURENCE F. FLICK, 519 Pine Street. Jefferson Medical College, 1879.
- 1881 (January). W. S. FORBES, 1409 Locust Street. Jefferson Medical College, 1882.
- 1883 (April). H. F. FORMAD, 3535 Locust Street. University of Pennsylvania, 1877.
- 1882 (April). JOHN K. FOULKROD, 1612 Richmond St. University of Pennsylvania, 1878.
- 1880 (June). C. W. FOX, 37 South Nineteenth Street. Long Island College Hospital, 1865.
- 1882 (October). L. WEBSTER FOX, 1306 Walnut Street. Jefferson Medical College, 1878.
- 1881 (January). MARCUS FRANKLIN, 1601 Columbia Avenue. Jefferson Medical College, 1870.
- 1879 (January). MORRIS STROUD FRENCH, 1423 Walnut Street. Jefferson Medical College, 1876.
- 1882 (October). H. H. FREUND, 1310 South Fifth Street. Jefferson Medical College, 1880.
- 1867 (July). WILLIAM S. FRICK, 821 North Eighth Street. Jefferson Medical College, 1848.
- 1849 (April). ALBERT FRICKÉ, 235 North Sixth Street. University of Berlin, 1839.
- 1881 (January). GEORGE FRIEBIS, 1505 North Eighth Street. Jefferson Medical College, 1879.
- 1883 (January). E. F. GARRETT, 5043 Main Street. Jefferson Medical College, 1876.
- 1879 (October). J. E. GARRETSON, 1537 Chestnut Street. University of Pennsylvania, 1869.
- 1852 (January). JAMES F. GAYLEY, 133 South Eighteenth Street. University of Pennsylvania, 1848.
- 1874 (October). FRANCIS H. GETCHELL, 1432 Spruce Street. Dartmouth Medical College, 1860; Jefferson Medical College, 1871.
- 1884 (January). JOS. S. GIBB, 841 North Sixth Street. University of Pennsylvania, 1880.
- 1883 (October). JOHN GILLESPIE, 1434 South Broad Street. University of Pennsylvania, 1880.
- 1879 (April). EDWIN R. GIRVIN, 714 North Nineteenth St. University of Pennsylvania, 1875.
- 1865 (April). ROBERT M. GIRVIN, 3934 Walnut Street. Jefferson Medical College, 1862.
- 1881 (January). W. H. H. GITHENS, 2033 Spruce Street. University of Pennsylvania, 1866.
- 1868 (January). J. B. HOWARD GITTINGS, 3435 Walnut Street. University of Pennsylvania, 1863.

Elected.

- 1881 (October). R. B. GLASGOW, 1441 N. Twentieth St. University of Pennsylvania, 1878.
- 1876 (April). WILLIAM GOODWELL, 500 N. Twentieth Street. Jefferson Medical College, 1854, and University of Pennsylvania, 1871.
- 1873 (July). JAMES GRAHAM, 1528 Spruce Street. Jefferson Medical College, 1867.
- 1881 (June). JOHN GRAHAM, 326 South Fifteenth Street. Jefferson Medical College, 1867.
- 1881 (April). ANDREW GRAYDON, 1249 North Fifteenth Street. Jefferson Medical College, 1877.
- 1884 (January). WILLIAM H. GREENE, 3225 Sansom Street. Jefferson Medical College, 1873.
- 1878 (April). O. A. GROFF, 215 N. Thirtieth Street. Jefferson Medical College, 1875.
- 1872 (January). FERDINAND H. GROSS, 1416 Girard Avenue. Jefferson Medical College, 1855.
- 1876 (January). SAMUEL W. GROSS, 1112 Walnut Street. Jefferson Medical College, 1857.
- 1869 (April). JOHN H. GROVE, 1330 Arch Street. University of Pennsylvania, 1849.
- 1884 (April). W. H. HALE, 1339 Arch St. Jefferson Medical College, 1882.
- 1870 (April). A. DOUGLAS HALL, 1623 Spruce Street. Jefferson Medical College, 1854.
- 1876 (October). JOHN C. HALL, Friends' Asylum for the Insane, Frankford. University of Pennsylvania, 1868.
- 1875 (January). L. BREWER HALL, 17 So. Sxteenth Street. University of Pennsylvania, 1873.
- 1882 (January). ROBERT H. HAMILL, 1812 Christian Street. University of Pennsylvania, 1878.
- 1859 (April). GEORGE HAMILTON, 1600 Summer Street. University of Pennsylvania, 1831.
- 1880 (April). B. F. HAMMELL, 3400 Spruce Street. University of Pennsylvania, 1863.
- 1883 (October). HOWARD F. HANSELL, 254 South Sixteenth Street. Jefferson Medical College, 1879.
- 1876 (January). GEORGE O. HARLAN, 1515 Walnut Street. University of Pennsylvania, 1858.
- 1860 (January). LEWIS D. HARLOW, 112 North Seventeenth Street. University of Pennsylvania, 1845.
- 1881 (April). RICHARD H. HARTE, 332 South Seventeenth Street. University of Pennsylvania, 1878.
- 1884 (April). MILTON B. HARTZELL, 3721 Spring Garden Street. Jefferson Medical College, 1877.
- 1880 (June). HENRY D. HARVEY, 4646 Germantown Avenue. University of Pennsylvania, 1878.

Elected.

- 1868 (July). NATHAN HATFIELD, 501 Franklin Street. Jefferson Medical College, 1868.
- 1850 (January). NATHAN L. HATFIELD, 501 Franklin Street. Jefferson Medical College, 1822.
- 1865 (April). THOMAS HAY, 1127 Arch Street. University of Pennsylvania, 1861.
- 1881 (January). I. MINIS HAYS, 266 South Twenty-first Street, and 1004 Walnut Street. University of Pennsylvania, 1868.
- 1877 (October). F. B. HAZEL, 855 North Eleventh Street. University of Pennsylvania, 1869.
- 1873 (July). W. JOSEPH HEARN, 312 Catharine Street. Jefferson Medical College, 1867.
- 1867 (April). EDWIN HELLYER, 816 Otis Street. Long Island College Hospital, 1864.
- 1878 (January). FREDERICK PORTEUS HENRY, 721 Pine Street. College of Physicians and Surgeons, New York, 1868.
- 1853 (April). ADDINELL HEWSON, 801 South Fifteenth Street. Jefferson Medical College, 1850.
- 1881 (January). ADDINELL HEWSON, JR., 1804 Pine Street. Jefferson Medical College, 1879.
- 1877 (July). ALBERT G. HEYL, 1535 Pine Street. University of Pennsylvania, 1870.
- 1867 (January). NAPOLEON HICKMAN, 326 South Sixteenth Street. University of Pennsylvania, 1862.
- 1867 (April). ELMORE C. HINE, 1834 Green Street. Yale College, 1861.
- 1864 (April). ALBERT G. B. HINKLE, 1300 Spring Garden Street. University of Pennsylvania, 1857.
- 1883 (April). A. B. HIRSH, 2130 Master Street. Jefferson Medical College, 1882.
- 1883 (October). WILLIAM H. HOCH, 1441 North Twentieth Street. University of Pennsylvania, 1880.
- 1890 (April). W. C. HOLLOPETER, 1408 North Thirteenth Street. University of Pennsylvania, 1877.
- 1884 (January). EDMUND W. HOLMES, 1523 Green Street. University of Pennsylvania, 1880.
- 1868 (January). JACOB FARNUM HOLT, 1985 Poplar Street. University of Pennsylvania, 1869.
- 1861 (January). CALBB W. HORNOR, Southeast corner Seventeenth and Walnut Streets. Jefferson Medical College, 1849.
- 1876 (January). CYRUS D. HOTTENSTEIN, 3306 Lancaster Avenue. Jefferson Medical College, 1848.
- 1879 (October). DANIEL E. HUGHES, 52 North Thirteenth Street. Jefferson Medical College, 1878.

Elected.

- 1880 (June). DONNEL HUGHES, 17 South Fortieth Street. University of Pennsylvania, 1879.
- 1880 (October). R. S. HUIDEKOPER, 111 South Twentieth Street. University of Pennsylvania, 1877. Alfort, France, 1882.
- 1881 (June). A. H. HULSHIZER, 1419 Otis Street. Jefferson Medical College, 1878.
- 1876 (January). WILLIAM HUNT, 1300 Spruce Street. University of Pennsylvania, 1849.
- 1868 (July). JAMES H. HUTCHINSON, 128 South Twenty-second Street. University of Pennsylvania, 1858.
- 1881 (October). F. S. ISATT, 525 North Sixth Street. Jefferson Medical College, 1876.
- 1878 (January). WILLIAM S. JANNY, 1535 North Broad Street. Pennsylvania Medical College, 1854, and Jefferson Medical College, 1880.
- 1882 (April). EDGAR P. JEFFERIS, 2029 Vine Street. University of Pennsylvania, 1878.
- 1878 (June). RUSSELL H. JOHNSON, 2109 Spruce Street. University of Pennsylvania, 1871.
- 1878 (October). LEONARD D. JUDD, 3603 Powelton Avenue. Jefferson Medical College, 1877.
- 1883 (January). LOUIS JURIST, 1210 Wallace Street. Jefferson Medical College, 1880.
- 1876 (January). JOHN M. KEATING, 1504 Walnut Street. University of Pennsylvania, 1873.
- 1876 (January). WILLIAM V. KEATING, 1604 Locust Street. University of Pennsylvania, 1844.
- 1890 (January). WILLIAM W. KERN, 1729 Chestnut Street. Jefferson Medical College, 1862.
- 1879 (October). W. G. KEIR, 28 North Thirty-eighth Street. Pennsylvania Medical College, 1846.
- 1881 (January). JOSEPH V. KELLY, 4257 Main Street, Manayunk. Jefferson Medical College, 1868.
- 1872 (October). GEORGE KERR, 711 South Nineteenth Street. University of Pennsylvania, 1864.
- 1883 (October). ROBERT O. KEVIN, 1156 South Tenth Street. Jefferson Medical College, 1882.
- 1873 (January). PETER DIRK KEYSER, 1630 Arch Street. University of Jena, 1864.
- 1882 (June). ROBERT KILDUFFE, 754 South Twelfth Street. Jefferson Medical College, 1880.
- 1880 (June). M. F. KIRKBRIDE, 125 South Sixteenth Street and 2212 Green Street. University of Pennsylvania, 1874.
- 1881 (October). M. BALDWIN KIRKPATRICK, 515 Brown Street. Jefferson Medical College, 1878.

Elected.

- 1849 (January). JOSEPH KLAFF, 622 Spruce Street. University of Pennsylvania, 1839.
- 1866 (October). WILLIAM O. KLINE, JR., 1213-15 Germantown Avenue. University of Pennsylvania, 1864.
- 1871 (Jan'y). SAMUEL ROBINSON KNIGHT, Episcopal Hospital. University of Pennsylvania, 1869.
- 1882 (April). HORACE LADD, 1225 Arch Street. Jefferson Medical College, 1848.
- 1882 (June). M. LAMPH, 3601 Spring Garden Street. Pennsylvania Medical College, 1860.
- 1882 (April). MAXIMILIAN LANDEBERG, 2006 Arch Street. University of Berlin, 1865.
- 1883 (October). EDWARD S. LAWRENCE, 622 South Sixteenth Street. Jefferson Medical College, 1879.
- 1887 (July). HENRY LEAMAN, 1033 Vine Street. Jefferson Medical College, 1864.
- 1883 (October). ROSE LEAMAN, 1033 Vine Street. Jefferson Medical College, 1882.
- 1867 (January). BENJAMIN LEE, Mannheim Street, Germantown. New York Medical College, 1866.
- 1881 (June). JOHN G. LEE, 333 South Twelfth Street. Jefferson Medical College, 1878.
- 1879 (April). HENRY LEFFMANN, 1230 Locust Street and 1330 Franklin Street. Jefferson Medical College, 1869. Pennsylvania College of Dental Surgery, 1884.
- 1860 (January). JOSEPH LEIDY, 1302 Filbert Street. University of Pennsylvania, 1844.
- 1876 (Jan'y). PHILIP LEIDY, 526 Marshall Street. University of Pennsylvania, 1859.
- 1877 (January). JEREMIAH R. LEVAM, 506 North Forty-first Street. University of Pennsylvania, 1861.
- 1849 (January). RICHARD J. LEVIS, 1601 Walnut Street. Jefferson Medical College, 1848.
- 1881 (January). G. W. LINN, Bryd Mawr. University of Pennsylvania, 1872.
- 1880 (October). WILLIAM S. LITTLE, 215 South Seventeenth Street. Bellevue Medical College, 1873. Jefferson Medical College, 1877.
- 1880 (April). ALFRED T. LIVINGSTON, 1704 Arch Street. University of Buffalo, 1873.
- 1884 (April). J. H. LLOYD, Fortieth and Walnut Streets. University of Pennsylvania, 1878.
- 1880 (October). P. E. LODER, 517 South Eighth Street. Jefferson Medical College, 1875.
- 1881 (June). G. LOKING, 1607 North Eighth Street. Jefferson Medical College, 1874.

Elected.

- 1883 (April). DANIEL LONGAKER, 601 Green Street. University of Pennsylvania, 1881.
- 1882 (April). MORRIS LONGSTREET, 1416 Spruce Street. University of Pennsylvania, 1869.
- 1878 (October). J. H. LOPEZ, 126 North Seventeenth Street. Jefferson Medical College, 1876.
- 1882 (October). LOUIS F. LOVE, 1818 Frankford Avenue. Jefferson Medical College, 1881.
- 1879 (June). J. L. LUDLOW, 1931 Chestnut Street. University of Pennsylvania, 1841.
- 1869 (January). WILLIAM LYONS, 1312 North Front Street. Pennsylvania Medical College, 1866.
- 1881 (April). JOHN MACAVOY, 1363 N. Second Street. Pennsylvania Medical College, 1841.
- 1881 (April). I. MACBRIDE, 1761 Frankford Avenue. University of Pennsylvania, 1864.
- 1882 (April). ALEXANDER WATT MACCOTY, 1417 Walnut Street. University of Pennsylvania, 1870.
- 1881 (January). N. G. MACOMBER, 4673 Germantown Avenue. University of Pennsylvania, 1871.
- 1881 (June). J. MARTIN, 2027 Columbia Avenue. Jefferson Medical College, 1878.
- 1881 (October). G. BETTON MASSEY, 1502 Arch Street. University of Pennsylvania, 1876.
- 1867 (January). ALEXANDER H. MCADAM, Southeast corner Fifth and Brown Streets. University of Pennsylvania, 1863.
- 1883 (June). WM. M. MCALARNRY, 1104 Vine Street. Jefferson Medical College, 1870.
- 1875 (July). CHARLES A. MCCALL, 3941 Chestnut Street. University of Pennsylvania, 1858.
- 1881 (June). GEORGE MCCLELLAN, 1352 Spruce St. Jefferson Medical College, 1870.
- 1881 (June). R. MILLER MCCLELLAN, 72d and Woodland Avenue. Jefferson Medical College, 1879.
- 1874 (October). COCHRAN MCCLELLAND, 516 South Eleventh Street. Jefferson Medical College, 1873.
- 1881 (January). S. M. MCCOLLIN, 1128 Arch Street. Jefferson Medical College, 1878.
- 1876 (April). R. S. MCCOMBS, 648 North Eleventh Street. University of Pennsylvania, 1868.
- 1880 (April). G. Y. MCCRACKEN, 612 N. Thirteenth St. University of Pennsylvania, 1876.
- 1881 (January). S. B. McDOWELL, 1128 Vine Street. Jefferson Medical College, 1876; University of Pennsylvania, 1879.

Elected.

- 1878 (October). B. F. McELROY, 1945 Christian Street. University of Pennsylvania, 1877.
- 1877 (January). JAMES A. McFERRAN, 1924 Green Street. Jefferson Medical College, 1847.
- 1880 (April). CHARLES H. McILWAINE, Trenton. University of Pennsylvania, 1877.
- 1868 (April). HENRY D. McLEAN, 1331 Pine Street. Jefferson Medical College, 1864.
- 1871 (January). THOS. A. McREAN, 625 North Seventh Street. Philadelphia College of Medicine, 1850.
- 1882 (January). J. EWING MEARS, 1429 Walnut Street. Jefferson Medical College, 1865.
- 1880 (October). A. V. MEIGS, 1322 Walnut Street. University of Pennsylvania, 1871.
- 1875 (January). CHRISTOPHER H. MILLER, 629 North Eleventh Street. University of Marburg, Germany, 1845, and Jefferson Medical College, 1850.
- 1882 (October). D. J. MILTON MILLER, 130 South Fifteenth Street. University of Pennsylvania, 1878.
- 1884 (April). JOHN S. MILLER, 834 North Nineteenth Street. Jefferson Medical College, 1882.
- 1875 (January). CHARLES K. MILLS, 113 South Nineteenth Street. University of Pennsylvania, 1869.
- 1876 (January). A. K. MINICH, 145 Susquehanna Avenue. Jefferson Medical College, 1870.
- 1878 (October). S. WEIR MITCHELL, 1524 Walnut Street. Jefferson Medical College, 1860.
- 1876 (October). EDWARD E. MONTGOMERY, 1305 North Broad Street. Jefferson Medical College, 1874.
- 1880 (April). W. P. MOON, Pennsylvania Hospital for the Insane. Philadelphia Medical College, 1869.
- 1876 (January). GEORGE R. MOREHOUSE, 227 South Ninth Street. Jefferson Medical College, 1850, and University of Pennsylvania, 1875.
- 1858 (April). JAMES CHESTON MORRIS, 1514 Spruce Street. University of Pennsylvania, 1854.
- 1883 (April). WILLIAM H. MORRISON, Holmesburg. University of Pennsylvania, 1880.
- 1882 (June). WILLIAM MOSS, Chestnut Avenue, corner Main Street, Chestnut Hill. Jefferson Medical College, 1855.
- 1881 (April). FRANCIS MUELENBERG, 1909 Chestnut Street. University of Pennsylvania, 1887.
- 1880 (June). J. H. MUSSER, 3705 Powelton Avenue. University of Pennsylvania, 1877.
- 1877 (July). CHARLES BYRLARD NANCORDE, 2109 Pine Street. University of Pennsylvania, 1869, and Jefferson Medical College, 1883.

Elected.

- 1870 (April). JOSEPH D. NASH, 1316 North Eleventh Street. Jefferson Medical College, 1865.
- 1855 (July). ANDREW NESINGER, 1018 South Second Street. University of Pennsylvania, 1850.
- 1880 (June). JOS. S. NEFF, 214 South Fifteenth Street. Jefferson Medical College, 1875.
- 1883 (June). THOMAS R. NELSON, 346 South Fifteenth Street. University of Pennsylvania, 1880.
- 1859 (April). HENRY W. NEWCOMBT, 924 North Broad Street. University of Pennsylvania, 1855.
- 1884 (January). B. F. NICHOLLS, 719 Spruce St. Jefferson Medical College, 1875.
- 1871 (April). EDWARD J. NOLAN, 830 North Twentieth Street. University of Pennsylvania, 1867.
- 1876 (April). GERALD D. O'FARRELL, 716 East Cumberland Street. University of Pennsylvania, 1862.
- 1867 (April). MICHAEL O'HARA, 31 South Sixteenth Street. University of Pennsylvania, 1862.
- 1879 (January). CHARLES A. OLIVER, 1507 Locust Street. University of Pennsylvania, 1876.
- 1881 (January). F. X. O'NEILL, 455 North Fourth Street. University of Pennsylvania, 1876.
- 1873 (July). JOHN J. O'NEILL, 1907 Vine Street. University of Pennsylvania, 1871.
- 1852 (June). OWEN OSLER, 874 North Sixth Street. Jefferson Medical College, 1846.
- 1881 (June). LAMBERT OTT, 1531 North Seventeenth St. Jefferson Medical College, 1878.
- 1883 (January). JOHN J. OWEN, 411 Pine Street. Jefferson Medical College, 1878.
- 1876 (January). JOHN H. PACKARD, 1924-26 Spruce Street. University of Pennsylvania, 1853.
- 1884 (June). HENRY O. PAIST, 623 North Sixth Street. Medical Department Penna. College, 1854.
- 1865 (October). WILLIAM H. PANCOAST, 1100 Walnut Street. Jefferson Medical College, 1856.
- 1876 (April). WILLIAM HENRY PARISH, 1612 Pine Street. Jefferson Medical College, 1870.
- 1878 (April). J. R. PARTENHEIMER, 653 North Tenth Street. University of Pennsylvania, 1872.
- 1878 (June). J. S. PEARSON, 1506 Christian Street. University of Pennsylvania, 1876.
- 1874 (April). JOSIAH PELTZ, 1617 Fairmount Avenue. University of Pennsylvania, 1867.
- 1871 (January). WILLIAM PEPPER, 1811 Spruce Street. University of Pennsylvania, 1864.

Elected.

- 1871 (January). EDWIN STANLEY PERKINS, 1901 North Twelfth Street. University of Pennsylvania, 1889.
- 1879 (April). F. M. PERKINS, 1428 Pine Street. University of Pennsylvania, 1878.
- 1882 (June). HERT M. PERRY, 3501 Hamilton Street. Jefferson Medical College, 1872.
- 1887 (October). WILLIAM CORNELIUS PHELPS, 2053 Vine Street. Pennsylvania Medical College, 1856.
- 1882 (June). GEORGE A. PIERSON, 1110 Spring Garden Street. University of Pennsylvania, 1877.
- 1875 (April). WILLIAM G. PORTER, 1223 Spruce Street. University of Pennsylvania, 1868.
- 1881 (October). J. B. POTSDAMER, 1629 North Eighth St. Jefferson Medical College, 1879.
- 1867 (January). CLAUDIUS R. PRALL, 1509 North Thirteenth Street. Jefferson Medical College, 1856.
- 1875 (January). MORDECAI PRICE, 313 North Ninth Street. University of Pennsylvania, 1869.
- 1884 (April). MCCLUNEY RADOLIFFE, 711 North Sixteenth Street. University of Pennsylvania, 1882.
- 1882 (June). CHARLES B. RANOK, 529 South Eleventh Street. University of Pennsylvania, 1872.
- 1882 (January). B. ALEXANDER RANDALL, 1806 Chestnut Street. University of Pennsylvania, 1880.
- 1883 (June). A. W. RANSLEY, 1230 South Tenth Street. University of Pennsylvania, 1876.
- 1884 (April). N. W. RAYNOR, 1324 Vine Street. University of Pennsylvania, 1879.
- 1875 (April). ALFRED GRAHAM REED, 228 North Twelfth Street. University of Pennsylvania, 1868.
- 1879 (January). GEORGE A. REX, 2118 Pine Street. University of Pennsylvania, 1868.
- 1876 (October). OLIVER PAYSON REX, 1611 Race Street. Jefferson Medical College, 1867.
- 1878 (April). T. C. RICH, 610 South Sixteenth Street. Georgetown College, Washington, D. C., 1869, and Jefferson Medical College, 1878.
- 1881 (January). ELLIOTT RICHARDSON, 1930 Spruce Street. University of Pennsylvania, 1867.
- 1876 (January). JOSEPH GIBBONS RICHARDSON, 3238 Chestnut Street. University of Pennsylvania, 1862.
- 1860 (April). WILLIAM M. L. RICKARDS, 601 North Seventeenth Street. University of Pennsylvania, 1843.
- 1875 (July). SAMUEL DOTY RISLEY, 1630 Walnut Street. University of Pennsylvania, 1870.

Elected.

- 1879 (October). A. S. ROBERTS, 131 South Fifteenth Street. University of Pennsylvania, 1877.
- 1876 (October). JOHN BINGHAM ROBERTS, 1118 Arch Street. Jefferson Medical College, 1874.
- 1883 (January). ROBERT P. ROBINS, 2226 Locust Street. University of Pennsylvania, 1880.
- 1860 (January). ROBERT E. ROGERS, Continental Hotel. University of Pennsylvania, 1836.
- 1882 (October). EDW. ROSENTHAL, 517 Pine Street. Jefferson Medical College, 1880.
- 1883 (April). BENJAMIN J. RUDDEROW, 527 North Nineteenth Street. University of Pennsylvania, 1874.
- 1884 (April). W. V. RUNKLE, 2003 Christian Street. Jefferson Medical College, 1874.
- 1880 (October). CHAS. F. SAJOUS, 1630 Chestnut Street. Jefferson Medical College, 1878.
- 1870 (January). EUGENE IRVING SANTEE, 605 N. Eleventh Street, and 532 North Sixth Street. University of Pennsylvania, 1866.
- 1877 (October). CHARLES SCHAEFFER, 1309 Arch Street. University of Pennsylvania, 1859.
- 1881 (January). PHILIP M. SCHIEDT, 1708 North Seventh Street. University of Pennsylvania, 1877.
- 1877 (January). HENRY S. SCHELL, 1802 Chestnut Street. University of Pennsylvania, 1857.
- 1875 (July). JOSEPH D. SCHOALES, 1428 North Eleventh Street. University of Pennsylvania, 1857.
- 1876 (January). ARNOLD SCHOTT, 1224 N. Seventh Street. Jefferson Medical College, 1868.
- 1884 (April). PETER N. K. SCHWENK, 606 Marshall Street. University of Pennsylvania, 1882.
- 1883 (April). GEO. DE SCHWEINITZ, 1330 Spruce Street. University of Pennsylvania, 1881.
- 1877 (April). CARL SEILER, 1346 Spruce Street. University of Pennsylvania, 1871.
- 1884 (April). C. JAY SELTZER, 111 North Sixteenth Street. University of Pennsylvania, 1881.
- 1880 (October). CHAS. M. SELTZER, 1631 Green Street. University of Pennsylvania, 1878.
- 1878 (June). THEODORE H. SEYFERT, 1709 Mount Vernon Street. University of Pennsylvania, 1867.
- 1876 (October). EDWARD ORAM SHAKE-SPEARE, 1336 Spruce Street. University of Pennsylvania, 1869.
- 1855 (July). ELISHA BEULAH SHAPLIGH, 658 North Eighth Street. University of Pennsylvania, 1849.

Elected.

- 1871 (April). GEORGE FRANCIS SHATTUCK, 1232 South Tenth Street. Medical Department Harvard University, 1862.
- 1882 (June). B. T. SHIMWELL, 1235 South Seventeenth Street. Jefferson Medical College, 1875.
- 1877 (January). JOHN V. SHOENMAKER, 1031 Walnut Street. Jefferson Medical College, 1874.
- 1883 (October). THOMAS SHRINER, 2204 Frankford Ave. Jefferson Medical College, 1869.
- 1882 (January). J. HENRY C. SIMES, 2033 Chestnut Street. University of Pennsylvania, 1870.
- 1882 (April). T. WALLACE SIMON, 129 S. Thirteenth Street. Jefferson Medical College, 1877.
- 1876 (April). JOSEPH S. SIMSON, 835 N. Eighth Street. Jefferson Medical College, 1874.
- 1881 (January). WHARTON SINKLER, 1534 Pine Street. University of Pennsylvania, 1868.
- 1881 (June). P. G. SKILLERN, 427 South Broad Street. University of Pennsylvania, 1877.
- 1863 (May). SAMUEL RUSH SKILLERN, 3416 Baring Street. University of Pennsylvania, 1854.
- 1875 (January). MICHAEL J. SKILLING, 1702 Christian Street. Jefferson Medical College, 1873.
- 1883 (June). H. A. SLOCUM, 1208 Spruce Street. University of Pennsylvania, 1879.
- 1853 (April). JOHN HENRY SMALTZ, 801 North Sixth Street. University of Pennsylvania, 1850.
- 1876 (January). ALBERT H. SMITH, 1419 Walnut Street. University of Pennsylvania, 1856.
- 1881 (June). H. A. SMITH, 1319 North Fifteenth Street. University of Pennsylvania, 1875.
- 1862 (April). HENRY H. SMITH, 1800 Spruce Street. University of Pennsylvania, 1837.
- 1881 (June). L. W. STEINBACH, 716 Franklin Street. Jefferson Medical College, 1880.
- 1881 (June). H. W. STELWAGEN, 223 South Seventeenth Street. University of Pennsylvania, 1875.
- 1883 (January). DAVID D. STEWART, 3929 Pine Street. Jefferson Medical College, 1879.
- 1883 (June). FRANÇOIS EDWARD STEWART, 731 South Twenty-second Street. Jefferson Medical College, 1879.
- 1870 (April). WILLIAM S. STEWART, 1801 Arch Street. Jefferson Medical College, 1863.
- 1849 (January). ALFRED STILLÉ, 3900 Spruce Street. University of Pennsylvania, 1836.
- 1869 (January). LUTHER K. STINE, 1502 North Fourth Street. University of Pennsylvania, 1860.

Elected.

- 1870 (October). A. E. STOCKER, 2212 Fitzwater Street. University of Pennsylvania, 1840.
- 1881 (June). EDW. R. STONE, 1539 North Nineteenth Street. Jefferson Medical College, 1872.
- 1878 (October). JAMES F. STONE, 1922 Mt. Vernon Street. Bellevue Medical College, 1866.
- 1876 (January). GEORGE STRAWBRIDGE, 1500 Walnut Street. University of Pennsylvania, 1866.
- 1883 (January). I. P. STRITTMATTER, 1232 North Fifth Street. Jefferson Medical College, 1880.
- 1871 (April). GEORGE EASTMAN STUBBS, Northeast corner Seventeenth and Jefferson Sts. Medical Department Harvard University, 1873.
- 1880 (January). C. STYER, 2201 Columbia Avenue. University of Pennsylvania, 1862.
- 1878 (June). J. H. TAYLOR, 1133 Spruce Street. University of Pennsylvania, 1852.
- 1880 (June). J. M. TAYLOR, Twenty-second and St. Alban's Place. University of Pennsylvania, 1878.
- 1870 (October). WILLIAM TERRY TAYLOR, 1324 North Fifteenth Street. University of Pennsylvania, 1848.
- 1879 (April). CHARLES HERMON THOMAS, 1807 Chestnut Street. University of Pennsylvania, 1865.
- 1875 (April). WILLIAM THOMSON, 1426 Walnut Street. Jefferson Medical College, 1855.
- 1878 (October). B. TRAUTMANN, 529 North Fourth Street. Georgetown University, D. C., 1874.
- 1871 (April). DENNIS J. TREACY, 1914 Christian Street. Jefferson Medical College, 1867.
- 1860 (January). SAMUEL N. TROTH, 2043 Franklin Street. Jefferson Medical College, 1849.
- 1878 (April). C. S. TURNBULL, 1702 Chestnut Street. University of Pennsylvania, 1871.
- 1849 (April). LAURENCE TURNBULL, 1502 Walnut Street. Jefferson Medical College, 1845.
- 1874 (April). JAMES TYSON, 1506 Spruce Street. University of Pennsylvania, 1863.
- 1880 (April). EDWARD S. VANDERSLOOT, 127 South Fifth Street. University of Pennsylvania, 1864.
- 1867 (October). EDWARD B. VANDYKE, 306 South Tenth Street. University of Pennsylvania, 1856.
- 1883 (June). A. VAN HARLINGEN, 129 S. Fifteenth Street. University of Pennsylvania, 1867.
- 1883 (June). C. H. VINTON, 1617 Race Street. University of Pennsylvania, 1868.

Elected.

- 1879 (June). GEORGE W. VOGLER, 565 N. Fifth Street. University of Pennsylvania, 1876.
- 1880 (April). JAS. W. WALK, 748 North Twentieth Street. University of Pennsylvania, 1878.
- 1881 (April). JAMES B. WALKER, 1817 Green Street. University of Pennsylvania, 1872.
- 1880 (April). ELLERSLIE WALLACE, 1130 Spruce Street. Jefferson Medical College, 1843.
- 1883 (October). ARTHUR W. WATSON, 264 North Twenty-first Street. University of Pennsylvania, 1880.
- 1882 (June). EDW. W. WATSON, 181 North Twentieth Street. University of Pennsylvania, 1875.
- 1881 (April). W. F. WAUGH, 1521 Arch Street. Jefferson Medical College, 1871.
- 1871 (July). WILLIAM H. WEBB, 556 North Sixteenth Street. Jefferson Medical College 1866.
- 1883 (January). LOUIS WEBER, 1538 South Sixth Street. University of New York, 1880.
- 1867 (January). JACOB H. WENNER, 156 Wister Street, Germantown. Pennsylvania Medical College, 1861.
- 1882 (October). F. LE SIEMUS WEIR, 860 East Thompson Street. Jefferson Medical College, 1876.
- 1865 (April). WILLIAM M. WELCH, 1214 Fairmount Avenue. University of Pennsylvania, 1859.
- 1865 (April). JAMES RALSTON WELLS, 5154 Lancaster Avenue. Jefferson Medical College, 1856.
- 1867 (April). R. HENRY WENVILL, 1010 South Third Street. Jefferson Medical College, 1863.
- 1881 (January). H. R. WHARTON, 1405 Locust Street. University of Pennsylvania, 1876.
- 1881 (January). R. S. WHARTON, 310 South Tenth Street. Jefferson Medical College, 1876.
- 1881 (January). E. B. WHEELER, 1926 North Eighth Street. Bellevue Medical College, 1873.
- 1878 (January). ALFRED WHELEN, 123 South Twentieth Street. University of Pennsylvania, 1874.
- 1878 (October). J. W. WHITE, 222 South Sixteenth Street. University of Pennsylvania, 1871.
- 1861 (October). JOHN E. WHITESIDE, Haverford Avenue, near Sixty-sixth Street. Pennsylvania Medical College, 1847.
- 1882 (April). EUGENE WILEY, 830 Reed Street. Jefferson Medical College, 1869.

Elected.

- 1879 (April). DE FOREST WILLARD, 1818 Chestnut Street. University of Pennsylvania, 1867.
- 1880 (June). HORACE WILLIAMS, 1717 Pine Street. University of Pennsylvania, 1866.
- 1881 (January). CHARLES H. WILLITS, 1839 Arch Street. University of Pennsylvania, 1879.
- 1883 (February). BENJAMIN B. WILSON, 1903 Chestnut Street. University of Pennsylvania, 1850.
- 1884 (April). C. MEIGS WILSON, 1517 Walnut Street. Jefferson Medical College, 1882.
- 1860 (April). ELLWOOD WILSON, 1517 Walnut Street. Jefferson Medical College, 1845.
- 1880 (June). H. AUGUSTUS WILSON, 1611 Spruce Street. Jefferson Medical College, 1879.
- 1876 (January). JAMES C. WILSON, 1437 Walnut Street. Jefferson Medical College, 1869.
- 1876 (January). JAMES FOSTER WILSON, 1010 Race Street. University of Pennsylvania, 1864.
- 1878 (June). CHARLES WINGMAN, 2005 Pine Street. Jefferson Medical College, 1877.
- 1883 (October). G. G. WISE, 543 South Broad Street. University of Pennsylvania, 1866.
- 1863 (April). CHARLES F. WITTIG, 480 North Fourth Street. University of Gottingen, 1833.
- 1875 (April). WINFIELD SCOTT WOLFORD, 1310 Walnut Street. Jefferson Medical College, 1873.
- 1874 (April). HORATIO C. WOOD, 1925 Chestnut Street. University of Pennsylvania, 1862.
- 1875 (April). FRANK WOODBURY, 218 South Sixteenth Street. Jefferson Medical College, 1873.
- 1879 (April). D. F. WOODS, 1501 Spruce Street. University of Pennsylvania, 1864.
- 1883 (January). JOHN L. YARD, 1608 North Twelfth Street. Jefferson Medical College, 1879.
- 1866 (January). THOMAS J. YARROW, 1335 North Broad Street. University of Pennsylvania, 1861.
- 1853 (April). GEORGE J. ZIEGLER, 123 Richmond Street. University of Pennsylvania, 1850.
- 1881 (June). WALTER M. L. ZIEGLER, 2007 Columbia Avenue. University of Pennsylvania, 1874.

DECEASED.

JOSHUA R. EVANS,	-	-	-	-	-	February 9, 1884.
SAMUEL D. GROSS,	-	-	-	-	-	May 6, 1884.
WILLIAM H. HOOPER,	-	-	-	-	-	December 18, 1883.
CHARLES T. HUNTER,	-	-	-	-	-	April 27, 1884.
THOMAS S. KIRKBRIDE,	-	-	-	-	-	December 17, 1883.
ANDREW S. McMURRAY,	-	-	-	-	-	April 7, 1884.
FRANK O. NAGLE,	-	-	-	-	-	February 1, 1884.
BENJAMIN PHISTER,	-	-	-	-	-	May 18, 1884.
ADAM F. SHELLY,	-	-	-	-	-	October 13, 1883.
FREDERICK A. SHEPPARD,	-	-	-	-	-	April 14, 1884.

PROCEEDINGS
OF THE
PHILADELPHIA COUNTY
MEDICAL SOCIETY.



PHILADELPHIA COUNTY MEDICAL SOCIETY.

DIETETICS FOR THE SICK.

Read September 12, 1883.

BY CHARLES M. SELTZER, M. D.

IN presenting this paper it is but justice to state that it has been prepared during the leisure of the past week as a substitute for one by another member, who was unable to keep his engagement. The subject has been selected more from the hope of eliciting some practical ideas, and a desire to emphasize its importance, than to announce anything new, or to exhibit the best paces of a hobby. Close attention to the methods of our most successful general practitioners, convinced me that in dietetics was their stronghold. At the same time there was plainly to be seen a distressing lack of knowledge as to variety of foods and the preparation of them. In order to find out whether there was a practical remedy for these two great faults, I have gone over a thorough course of preparation of diet for the sick, under the instructions and guidance of Mrs. S. T. Rorer, in whom are combined the practical attributes of cook, chemist, physiologist and nurse, and I can safely say that if every member of this society were to do likewise, their *armamentarium*, success, and self-satisfaction in the practice of medicine, would be increased many fold.

It would be almost an endless task to discuss the literature of this subject. Let us condense the opinions of physiologists, chemists, hygienists, and all others in authority, by stating that food is divided into nitrogenous or albuminous; hydro-carbons or starch, sugar and fats; and mineral or the various salts of the alkalies. Of these the nitrogenous and mineral are the building materials, and the hydro-carbons are the heat- and force-producers.

Food may also be classified into liquid, semi-solid and solid, and as the sum total of digestion is a matter of absorption and assimilation, it follows that the more liquid or dissolvable food is, the more readily will it be absorbed; hence, this is the form to be chosen when it is desirable to obtain nourishment at the least possible expense of organic action. There are also conditions in which bulk and solidity are necessary, and here we select the semi-solid or solid food, or even indigestible matter.

Nitrogenous food, in its natural combination with mineral matters, is mostly made absorbable by stomachic digestion—it rebuilds the tissues, and the waste and refuse are excreted by the kidneys and skin as urea and uric acid and its salts.

Hydro-carbons are digested in the mouth and small intestines, are burnt up by union with the respired oxygen, thus supplying heat and force to the body. The waste and refuse are excreted by the lungs and bowels.

About three ounces of nitrogenous and twenty ounces of carbonaceous food are necessary to sustain the life of an adult man in idleness. With these epitomized facts in mind, let us consider the importance and use of dietetics in the treatment of disease. The importance has been signalized by Bence Jones, who has said that the effect of diet is far beyond any known remedy. This fact has been so well known for the past century, that volume after volume has been written on "Dietetics," "Diet Cures," etc. A few of these have become standard works of authority, because of their scientific and exhaustive treatment of the subject, while hundreds of others have had but an ephemeral existence. All have had to reiterate the same facts, because very few new ones have been learned, and aside from the knowledge of artificially digested food, the student of a hundred years ago knew as well as the student of to-day about what diet was necessary for the sick. True, theirs was an empirical teaching; they knew nothing of the physiological actions of diastase, pepsin and pancreatin, but they were all the more practical for that. Our present scientific reasonings on digestion are but the formation of rules and theories for very old facts. The importance of the subject is made more manifest when we consider how much of the sickness that we are called upon to treat is the result of some error in eating or drinking, and as "removal of the cause" is the first step towards the promotion of recovery, it is easy to perceive how essential it is to know

what constitutes proper and improper diet. Life depends upon diet, and the restoration of health depends upon the same principles as its preservation. Disease is the result of the violation of the laws of health, hence the first step towards recovery is to re-establish those laws; and as the material for repair and support must come from diet, we, as thorough physicians, should have a complete *practical* knowledge of dietetics, and a proper appreciation of their curative value. But almost a decade of experience among physicians compels me to acknowledge that this is not the case, and that the most lamentable deficiency is in the practical application of the subject. Nature's simple and easily understood remedies are either lost sight of or ignored in the blind endeavor to find a medical specific for every human ill, or to establish an ideal notion, theory or pathy. For the treatment of chronic affections, some good points may occasionally be gained by even observing the *modus operandi* of quacks, homeoquacks, and proprietary medicines, for in connection with either a depurative or inoperative preparation, such dietetic and hygienic directions are almost invariably *insisted* upon as would, in themselves, relieve temporarily, and perhaps permanently, some of the most obstinate cases.

The methods of dietetic treatment of diseases are as various as are the ills that flesh is heir to, but all of them contain some of the following principles as essential features. In acute diseases, *rest* is of primary importance; it is the fundamental principle of their medical and surgical treatment, and the same idea must be carried out in dietetics by remembering what part each organ serves in absorption, assimilation and excretion, and by avoiding such food as would cause the performance of that function. Of course this plan is only to be pursued while the disease is advancing, and as soon as recovery begins, there must be a gradual return to a promiscuous diet, very little at first, just enough to slightly exercise the crippled organ, using as great care as we would in instituting passive motion in a limb that has been fractured. Nature enforces this principle by the loss of appetite that marks the onset of nearly all acute diseases. The dietetic rest of an organ does not necessarily imply a depletory diet, for while it is non-active, other organs may be made to perform a compensatory amount of work, and thus sustain or even increase the patient's vitality—so great is the versatility of our organic

functions. It is upon this idea that we must act when we obey the command of Graves to feed fevers. In chronic diseases—those in which the natural course is direct from bad to worse, until death or some interposing process puts an end to them—every organ must be made to do its utmost; the whole system must be awakened to the threatening destruction. Herein lies the broadest field of action for our subject. No time should be lost; food should be administered as frequently as assimilation and proper amount of organic rest will allow; the appetite should be kept active, by using liquid, semi-solid and solid food, prepared in such a variety of ways as to allow no charge of monotony or disgust. Surprise is frequently a useful element. Instead of leaving the patient to vacillate over his or her likes and dislikes, the nurse should be privately instructed how and what to prepare for each time, and how to serve it in the most appetizing manner. A valuable aid, usually lost sight of, is condiment. Think but for a moment how a savory dish will sharpen our appetites while in health, and I am sure you will perceive its stimulating influence upon a debilitated patient, to whom the flat and insipid preparations usually offered are loathsome or even nauseating. This disgust might frequently be avoided, and the amount of prescribed medicine be lessened. Many of the bitter tonics and carminatives of our materia medica are the same as are used in cookery, and are none the less efficacious in a palatable dish than in the nauseous pill or draught. Therefore let the delicate aroma of herbs, celery and bay leaves pervade the soups; use India curry on starchy foods, and when desirable add *Capsicum*, *Piper nigrum*, etc., to animal broths and substances. In so doing, if necessary, you can satisfy the qualms of your professional dignity by remembering that in catering to the palate you reflexly stimulate the organs of digestion.

Some articles of diet may be administered simply for their curative effect. A five-ounce cup of strong coffee contains about sixty-six grains of extract, or an equivalent of about two grains of *Caffeine*—quite sufficient to relieve a neuralgia, or a headache and sick stomach after a dose of opium. Beef-tea, made red-hot with red pepper, is the very best treatment for delirium tremens. A patient to whom I once administered such a dose, made so strong that I would not have dared to taste it myself, afterwards told me that it was the most refreshing and *cooling* drink he had

ever taken. A London surgeon to the police told me that he had treated a hundred and fifty cases of delirium tremens with this remedy *alone*, and had not lost one. The use of *Chloral* in these cases is criminal, and many a death certificate of "delirium tremens" ought to be "heart failure from chloral poisoning." Mucilaginous teas and drinks made from gelatine, isinglass, Irish moss and flaxseed are very soothing to any inflammation within the digestive track. Green vegetables are necessary to cure as well as prevent scurvy.

An old practitioner, who had spent his early professional life in the country, told me that when he first commenced practicing he used all the ordinary remedies in typhoid fever without any satisfactory results, until the introduction of turpentine by the late Prof. Geo. B. Wood. In it he soon discovered a useful ally, not so much in the drug *per se*, but because convenience and time compelled him to use a thick flour gruel as a vehicle, and the patients were nourished sufficiently to live through the attack.

In the science and art of preparing sick diet there is a most lamentable lack of knowledge, especially among physicians. They know the preparations by name, but not by nature, and the only way to learn the latter is to don the apron and take a practical course from a practical and scientific cook. Such a course was inaugurated last winter in this city and in Boston with a very satisfactory result. The prospect for this season in this city is that the course will be very well attended. It is the only way to learn. I could read you receipts by the score, but it would be as useless as reading off that many medical formulæ.

DISCUSSION ON DIETETICS FOR THE SICK.

Dr. L. Turnbull, in opening the discussion, said: In reference to these questions, my opinions are rather of the old-fashioned kind, and I believe in courses of instruction on cooking, which will be of much use to physicians. Dr. Benj. Rush was accustomed in his lectures to remark that "a physician should spend six months in a kitchen before entering upon his practical career." He desired to heighten the importance of a knowledge of the effects of food, and also the correct way of preparing it. All these advantages could be combined with a course on pharmacy. Physicians should learn how to make beef-tea, extract, etc., and similar articles. The important preparations of mush, of Indian and oat-meal, for instance, are rarely properly made. They should be prepared over night, so as to be

thoroughly gelatinized; if made in the morning just before being served, they will not be digestible. In cases of very weak stomachs, many articles of diet may be peptonized with great benefit by using a small amount of a mixture of sodium carbonate and pancreatin, these being allowed to act for a short time at a temperature of about 80° F. Fairchild, of New York, prepares powders containing these peptonized materials in convenient amount. Such peptonized foods are like the old-fashioned preparations made with rennet and warm milk, and will be well borne by delicate stomachs. An article that is often wrongly prepared is barley. It may be softened and extracted with water, or it may be torrefied so as to make a sort of malt extract. In the latter form it is very suitable as a substitute for the diastase-like principle of the saliva in cases where that secretion is deficient. It can easily be given mixed with other food, as in puddings, for instance.

This summer we have been very successful in relieving the irritability of the stomach and bowels of both children and adults, due to temporary indigestion, by substituting first barley-water alone, and after a time a wineglassful of barley-water, with half a tumbler of milk, or its equivalent of condensed milk.

Too little attention is given to character of water. It is sometimes very bad, containing either injurious mineral or vegetable matter. The Boston water supply was much injured lately by certain sponge forms.

Butter is an article of food which is often impure, and besides objectionable substitutes for it are often sold. The mixtures of animal fats known under the names of oleomargarine and butterine should not be used in the preparation of food.

Beef-tea contains often but little nourishment, and in debilitated conditions, such as most cases of typhoid fever, beef essence made, not from the round of beef, but from the neck, in which blood is found, will be much more nourishing. A valuable form of beef-juice can be prepared by cutting up in pieces fresh beef, and placing it under a block of ice until, by the pressure, all the blood has been extracted. With the addition of a little salt it has been found to be retained by the most delicate stomach, and is very useful in cases of great exhaustion.

Dr. Albert H. Smith: The older we get the less disposed we are to rely on medicine and the more on diet. The remarks in the paper in reference to the importance of teaching physicians the art of cooking, are deserving of full endorsement. On one point, however, I cannot agree with the author of the paper. He seemed to advocate the use of highly stimulating food in acute diseases; in other words, the methods advocated by Dr. Thos. King Chambers in his fascinating work—a work, however, which has done great injury. Dr. Chambers' idea was that all disease was due to depression, and therefore the remedy was to stimulate the system. This is an attractive theory, but is not true. In cases of disease attended with high temperature, we only add fuel to the flame by the use of stimulating food.

Nature has admirable powers of taking care of herself, even under a

starvation regimen. The use of a low form of diet, with refrigerants and depressants, may often be of the greatest benefit. I can look back on my early experience, and recall cases in which, after operative measures, patients have been treated on the theory that plenty of pabulum should be furnished in the system, but such patients have perished. A strong patient will in such cases do much better on a low diet. Adhesive inflammation will be secured more satisfactory on the low diet system.

Dr. J. M. Barton: I had under my charge during three years a patient who sustained life on about a pint of milk a day—never more than a quart a day. He was a physician who had his own views on questions of diet and therapeutics. Dr. A. H. Smith is right in his position as to the importance of low diet after operations, but in typhoid fever we have a very different indication. In the first case, *i. e.*, after operations, food frequently acts as an irritant, but in the low fevers the patient is exhausted and needs support.

Dr. Forbes: I am reminded of a compliment which I heard the younger Larrey pay to Orfila, the great French chemist. He said that he (Orfila) was the greatest cook that ever lived. He said Orfila understood the disintegrating power of hot water. The effect of thorough boiling is most important, and since the days of Washington it has been an article of war that beans should be boiled for six hours. In the Crimea marked difference was seen between the French and English troops on account of the superior attainments of the French officers and surgeons in reference to many of the details of a soldier's life, and especially in cooking.

Dr. Seltzer, in closing the discussion, said: I did not wish to include in the paper a discussion of the methods of preparing food, but merely to express the idea that it is important that doctors should know what to cook and how to cook. Many articles of food that as ordinarily prepared are unfit for the sick, can be cooked so as to be palatable and digestible. Mrs. Rorer's method of cooking liver is an instance of this. She exhausts it with water to remove blood, then places it on ice until required, when it is cut into thin slices and toasted; sweetbreads are somewhat similarly prepared.

As to the use of stimulating food, we must judge Dr. Chambers' views by his own method. He draws the line between acute and chronic diseases, not making time the basis of the classification, but the tendency of the disease. Acute diseases tend to recovery, and these he leaves undisturbed; chronic diseases tend to fatal results, and in these he uses a highly nutritious diet, stimulating when a depressed state exists.

In reference to the suggestions made by different speakers as to the importance of systematic instruction of physicians in the art of cookery, I may say that such courses are now being organized. I do not think that such instruction can be advantageously given with pharmaceutical teaching. A woman's taste and tact are essential to the art.

A CASE OF AMPUTATION OF THE BREAST, WITH
REMARKS.

Read September 12, 1888.

BY HENRY LEAMAN, M. D.

MRS. J. J. W., æt. 48, married twenty-nine years, is the mother of eight children still living, and has been the recipient of ten severe miscarriages. She presented herself at my office on the first of September, 1882, complaining of a tumor in her breast, which she had noticed for the first time three months previous. At that time a small pimple upon the surface of the skin served to call her attention to the swelling that lay beneath in the substance of her breast. Examination showed a tender enlargement, the size of an English walnut, situated deeply in the inner, lower quarter of the left breast. She stated that during the past week she had experienced for the first time frequent, sharp, retracting pains shooting through the nipple. Menstruation had ceased with her six years previous, without giving rise to any trouble. She now had no *cachexia*, and was apparently in her usual health.

On the eighteenth of the same month she called again; the growth had not perceptibly increased, but she spoke of having pain in her breast bone. Amputation of the breast was recommended. The effect of this advice was to send her upon a blind pilgrimage through the desert of therapeutics. Now electricity, now pow-wowing, now homœopathy—each in its turn was tried with results equally gratifying to the patient.

The *ignis fatuus* which I had lighted brought her again to me on the twenty-third of April, 1883. The tumor then was of an oval shape, and about four inches in length. From its inner anterior surface were extending two cornua, slightly ulcerated, about three-fourths of an inch in length. The skin over the tumor was deeply congested, red and inflamed, as was also the skin for several inches around. The tumor in front rested upon the cartilages of the rib, but was movable. The glands in the axilla were slightly involved. With the assistance of Drs. Hatfield, Brubaker, Welch and R. Leaman, the breast was removed under the spray, on April 26, 1883, and Lister's dressing applied.

One nodule of hardness was removed from the axilla. At the sternal end the cut surfaces could not be approximated within two inches, owing to the necessary ablation of tissue.

April 27.—Patient doing well. Temp. $99\frac{1}{2}^{\circ}$, pulse 112 at 10 A. M.

April 28.—Temp. 99° , pulse 104 at 10 A. M.

The breast was dressed under the spray at 10 P. M. Temp. 99° , pulse 104.

April 29.—Temp. $98\frac{1}{2}^{\circ}$, pulse 96 at 10 A. M.

April 30.—Temp. $99\frac{1}{2}^{\circ}$, pulse 96 at 10 A. M.

May 1.—Temp. $98\frac{1}{2}^{\circ}$, pulse 92.

May 2.—Temp. 97° , pulse 84; dressed the wound the second time under the spray; the drainage tube was removed, as were also some of the sutures.

May 3.—Temp. 97° , pulse 80 at 4 P. M.

May 4.—Temp. 97° , pulse 100. Dressed again under spray.

May 6.—Dressed under spray; removed the sutures and two ligatures. Temp. 98° , pulse 104.

May 7.—Temp. 98° , pulse 104. Patient sitting up.

May 8.—The posterior three-fourths of the incision entirely healed and healthy. The anterior one-fourth was perfectly healthy and granulating rapidly. All sutures and the remaining ligatures were removed.

On and after May 10 the wound was dressed with iodoform, cosmoline and salicylated cotton, under which treatment the wound rapidly healed. Dr. Brubaker made a histological study of the growth, and pronounced it carcinoma.

As soon as the cicatrix was complete, neuralgic pains began to be felt in the left arm, right leg and body. At first the cicatrix remained perfectly healthy in appearance. Subsequently, however, a nodule appeared in the lower part of the neck behind the left sterno-clavicular articulation. Next, the left axilla, the posterior and healthy part of the cicatrix, both began to show hardening. At present there is a chain of nodules along the whole cicatrix—one large and painful is felt over the cartilage of the third rib on the left side. The left axilla is but one hard pyramid. The left hand and arm are swollen. A pain of a lancinating, burning character is referred to the left scapula, arm and axilla, occasionally shooting in the course of the incision.

The apparent freedom from involvement of the axillary glands at the time of the operation, and the rapid development of the disease after the healing of the cicatrix, would seem to point to the idea that the original tumor was but a local cnidus for a constitutional trouble, and that this having been destroyed, a general

efflorescence of the disease ensued. A kindred idea is found in the popular belief that a manifestation of phthisis may follow the operation for the cure of anal fistule. The observations on the change of life in women would seem also to bear upon the same point.

Dr. Brubaker's Analysis of the Tumor.—"Upon section the tumor is firm and hard, and presents a white, glistening surface, from which can be scraped a small quantity of fluid matter. Under the microscope the connective tissue stroma is seen to be abundantly developed; in its meshes are developed epithelial cells, some of which have undergone degeneration. In some situations the cells are arranged in a linear manner, whilst in others they form groups or nests."

DISCUSSION ON AMPUTATION OF BREAST.

Dr. Baldwin: I remember that the late Dr. Atlee spoke of the effects of arsenic in carcinoma and gave a number of cases in which he used it with the apparent effect of preventing the return of the tumor. I employed it in a case on which operation was performed in 1875, the patient being put on the use of Fowler's solution, continued until 1876, when the growth redeveloped and a second operation was done in 1878; no return has occurred to this time. The use of arsenic was discontinued after the second operation. During the formation of the second growth some trouble occurred in the lung, and cancer of that organ was feared, but it disappeared after operation.

Dr. Forbes: Dr. Leaman speaks of a period of efflorescence after operation, by which I understand a general development in different organs; this, as he remarks, does not always occur after first operation. In 1864 I removed a cancer from the breast of a lady. In 1867 a hardened growth appeared in the cicatrix. It was not removed at once, as it was then giving no pain. In a short time, pain having developed, it was removed. It returned a number of times and was each time removed until it developed in attachment to the interosseous cartilage and the muscles, and then I declined to remove it, although the patient very much urged me to do so. It continued to increase, and in eighteen months it involved the integument on the clavicle and sternum. Its condition was much aggravated by various caustic applications. Finally some irregular parties attempted to remove parts of the diseased mass, and this was followed by an efflorescence and a number of organs in the thorax and abdomen became involved. The patient died eighteen months after the last attempted operation.

I have heard Dr. Geo. W. Norris speak of this condition of efflorescence and its likelihood to occur in patients upon whom the cauterizing and

escharotic treatment had been practiced. After such treatment he advised non-interference with the knife.

Dr. J. M. Barton: I have been interested in examining the advertisements and circulars of the empirics who claim to cure cancer without operating, and I find them reporting cures of cases of over forty years' duration. Now, as scirrhus if left untreated does not usually last more than four years, it must be that these long cases are benign tumors.

Even when the knife does not cure, it may prolong life—make it more tolerable. Operation offers us ten per cent. of recoveries. Dr. Leaman's case can be regarded as typical. The almost immediate return in the axilla is suggestive of the possibility that some affected gland had escaped removal. The removal of the entire breast in the earlier stages gives chance of total recovery, and in the later stages, with ulceration, operation is also useful; but in the intermediate periods, when the tumor is extensive and adherent to the adjacent structures, non-interference is best.

Dr. Albert H. Smith: I am glad to hear Dr. Barton advocate the repeated use of the knife; I believe in attacking the tumor every time it appears, for, at the least, life may be prolonged and made comfortable, and the patient may be spared the suffering and loathsome discharges of the advanced stages of the disease, conditions which make her a source of misery to herself and to those around her. Cancer is doubtless a local disease, although a constitutional taint or tendency is probably inherited. Yet, if the tumor be removed early, a cure can be obtained. Dr. Leaman's patient did not do well because she did not follow the advice he gave at first. Removal of the breast should be complete; every reappearance should be met by operation. I recall the case of a patient from whom, at the first operation, no axillary glands were removed, but at the second twelve were taken out. Two more operations were subsequently performed; at the last one a very large amount of tissue was removed, but perfect union was obtained. Operation should cease when the condition is such that union by the first intention cannot be obtained. In this particular I cannot agree with Dr. Gross, who pays no attention to the size of the cicatrix or the question of union by first intention. The patient just referred to has been well during this summer, but is not beginning to manifest infiltration, and the condition is such that primary union cannot be obtained, and I do not consider operation permissible. Under the modern advances in antiseptic surgery, operative interference in cases of cancer becomes simple and entails little suffering, and the after-treatment is not difficult.

Dr. Barton: The important point in operation is to make a complete cleaning out of the affected glands, and the obtaining of this must not be interfered with by consideration of the amount of tissue to be removed, or by the desire for primary union. Where much tissue is removed, the after-treatment is more simple than where flaps are made; no counter-openings are needed; no drainage tubes; no pockets of pus are formed; but little, if any, fever occurs; the wound usually goes on uninterruptedly

to full cicatrization. In hospital practice I rarely have to see a case after operation, from which the breast has been thus removed. I cannot recall a case in which the disease returned in the cicatrix so formed, though it often returns in the axilla. I remember a case in which three operations were performed upon secondary axillary growths in a period of four years—the last operation being amputation of the arm, ligation of the axillary artery, and removal of the growth—yet the resulting cicatrix from the first thorough breast operation remained perfectly soft and healthy.

Dr. Leaman. The case I have reported is an ordinary one, and the operation was certainly performed with ordinary care. The axilla was opened, and all present agreed that no glands were affected except the one removed. The first symptoms of return were shooting pains in the fingers, referable to a nodule near the insertion of the sterno-cleido-mastoid muscle, behind the left sterno-clavicular articulation. Subsequently the axillary glands were involved. A few small nodules have since appeared over the seat of the first tumor.

I think it important to remember that surgeons may make mistakes; that some of the cases diagnosed as cancer, and apparently cured by operation, may be benign tumors.

In my experience arsenic has not been of use in carcinoma.

A NEW USE FOR AN OLD INSTRUMENT.

Dr. Leaman called attention to the fact that he had found the hooked end of Dr. Gross's ear and nose instrument the best and safest means of rupturing the membranes in labor.

REMARKS ON THE CHICAGO BEEF WIRE SKEWER.

Read September 12, 1883.

BY WM. R. D. BLACKWOOD, M. D.

THE supply of good, wholesome meat to a large city is a problem involving many points of great importance. Many cattle are slaughtered within a few hours after reaching the abattoirs, before the feverishness and excitement resulting from a long railroad journey have abated, and the meat under the circumstances is not nearly so good or suitable for food as it would be even in healthy and prime animals, were it killed after a due time of rest. For a short time past beef slaughtered in Chicago and brought here in refrigerator cars, has attracted the attention of those able to judge the article according to its merits, and for quality it is

pronounced fully equal to any heretofore put on the market, and far ahead of the great bulk previously sold in the city so far as a wholesome, sound and moderate-priced beef is concerned. The animals are selected from approved droves, and well fed, watered and housed for a definite and proper time before killing, and the product therefore is not only in its appearance perfect, but in the vital point of fitness for wholesome, nutritious and palatable food it is unrivaled.

Some weeks ago a sensational attack was made on this variety of beef in the interest of a clique of butchers in this city, who, knowing the value of the Chicago article, were afraid of the effect on their business when it became better known to the public, and the silliest pretext, amongst others, advanced, was a supposed danger to consumers from the novel skewers employed to fasten the labels on the hind- and fore-quarters, these being of barbed wire such as I exhibit to you to-night. It was predicted that an epidemic of harpooned tongues, tonsils and pharynxes would ensue from swallowing unawares by consumers the numerous wire skewers concealed in the meat, and I am informed that for a time a serious falling off in sales actually resulted from fear on this point. You will readily see that any one who would try to gulp down a morsel of meat large enough to hide this fastener must at the same time be in a famishing condition, have the appetite of a tiger, and need lessons in table etiquette, to put it mildly.

The labels which are attached—one to each quarter only—are removed by the butcher before cutting up the meat, and could not, even if allowed to remain, fail to attract attention of both cook and eater. Mr. Bradley, of the Great Western Market, is the largest dealer in this city of this excellent beef, and he has kindly supplied me with samples of the barbs. The claims which he makes for the particular beef under consideration are worthy of notice, as his experience in business for some years past is unequaled in this city, and the enormous quantity which he distributes to a large section of surrounding country fully justifies the high value he places upon it. I am glad to confirm what he says, from personal experience in my family. The meat is simply delicious, and excels anything we have previously had from the best butchers of the city.

VERMIFORM MUCOUS DISCHARGE FROM THE
RECTUM.

Read September 19, 1883.

BY BENJAMIN LEE, M. D.

THE interest of this specimen lies in the close resemblance it bears to a tape-worm. This is by no means so striking as it was when I received it a week ago. It was sent to me, in fact, by the mother of the patient, a young lady residing in a neighboring city, under the impression that it was a portion of a tape-worm, partly dissolved by medicine. The history is briefly this: The patient had been under my care, some years since, for a uterine displacement with hemorrhoids. The latter had pretty much disappeared, but had been succeeded by hemorrhages from the bowels—not large in quantity or very frequent. About four weeks since, she noticed that she was passing segments of a tape-worm. Another member of the family having been recently infested in the same way, and having been successfully treated by the use of pumpkin-seed in milk, according to an old formula of Paul Beck Goddard's, which was among the family treasures, she determined to treat herself on the same plan. Accordingly, after a fast of thirty-six hours she took half a tumblerful of pounded kernels of pumpkin-seed, stirred into the same quantity of milk, and two hours after, three tablespoonfuls of castor oil. This brought away only segments of the intruder, alive and active. After an interval of thirty-six hours, during which she took but one meal, she repeated the two doses, with the same effect, except that towards the end of the purgation the segments appeared dead. The purgation, however, was soon followed by profuse and protracted hemorrhage from the bowels, which, in addition to the action of the medicine and the long abstinence from food, left her very much exhausted. The following week she visited me at my office. Examination with the finger showed retroversion and partial prolapse of the uterus, the shrunken remains of external piles, no internal piles or tumors of any kind, a thickening of the posterior wall of the rectum, and a velvety or fibrillous feel to the mucous membrane overlying this thickened portion. As frequent hemorrhages had occurred since that which followed the use of the tænicide, I strongly advised against any further immediate attempt

on the parasite. I instructed her to take an injection of ten grains of zinc sulphate in an ounce of flaxseed-tea at bedtime every night, to be retained, and an injection of a pint of flaxseed-tea the following morning to produce an easy evacuation.

A week later she sent me down this specimen, saying that she had passed several more like it, and wishing to know if it was not the worm partially dissolved. She again visited my office on the fifteenth of this month. There had then been no hemorrhage for five days, and whereas at first it was with the greatest difficulty she could retain the injections for an hour, she was now able to retain them all night with comparative comfort. The discharges of mucus, on one day amounting to five or six, did not now occur as often as daily. They are preceded by a sense of weight and tenesmus, much as the hemorrhages were, which is relieved after they have taken place. I omitted to say that I had also prescribed fifteen drops of fluid extract of ergot, four times daily. I now ordered a continuance of the injections, and a diminution of the ergot to ten drops. Upon examination the rectal mucous membrane felt decidedly more natural. I yesterday received a note from her saying that she had passed fifteen inches of the worm very close to the head, but could not make sure that the latter had come away. Should this prove not to be the case, I shall, when the mucous membrane of the rectum has become sufficiently repaired, administer pelletierine, having used it successfully quite recently. The patient, a gentleman living also out of town, writes me that the most notable effect of the medicine, apart from its dislodgment of the intruder, was "extreme dizziness, and a feeling of the eyes as if starting from the sockets, accompanied by uncertainty of vision." "The feeling of nausea," he adds, "was, however, by no means of the distressing nature that I told you accompanied a previous attempt upon the worm." The attempt he refers to was made, I think, in Germany, and the remedy used was the pomegranate; and so intense was his suffering from deathly nausea, that he has been content to play host to his not very troublesome guest, now for some years, rather than undergo a similar experience of wretchedness, being only driven to it finally by his approaching nuptials.

SPECIMEN OF ANEURISM OF THE ASCENDING POSTERIOR PART OF THE AORTA.

Read September 19, 1883.

BY CHAS. M. SELTZER, M. D.

I AM indebted to Dr. W. Duffield Robinson, Resident Physician of the Eastern State Penitentiary, for the pleasure of being allowed to exhibit this specimen. It is of especial interest to me because I saw the case several times during life.

L—, aged 35 years, height 5 feet 7 inches, usual weight 142 pounds, was admitted to the Eastern State Penitentiary, under a two years' sentence for larceny, in September, 1882, previous to which time he had been four months in a county jail. Sixteen years of his life were spent in various prisons under different sentences. No family history could be obtained.

His medical history dates back sixteen months ago, when he complained of pain between the shoulders, but he continued in comparatively good health until six months ago, when he began to complain of diffuse intercostal pains in both sides and pain in the epigastrium; also poor appetite, nausea and indigestion; anæmia and general debility. Upon careful examination, no organic disease was discovered. The heart was rather weak, but otherwise normal. Thoracic percussion was normal, except slight increased dulness under upper part of the sternum. Pulse feeble, but otherwise normal. There was no syphilitic history, no œdema, no palpitation of the heart or præcordial pains, and no interscapular pain during the past year. No bulging of the chest-walls, either anteriorly or posteriorly. No discoverable aneurismal bruit. In fact there was nothing to indicate any organic disease. The case was examined by six reputable physicians, and all of them arrived at the same negative conclusion. The patient's physical condition steadily grew worse, and he was given the liberty of a large yard, excused from work, and allowed a plentiful nutritious diet, general tonics, and anodynes for the neuralgias. Under this treatment he seemed to improve, but the neuralgias and insomnia continued.

Three weeks ago (August 28, 1883), while sitting in the yard, he suddenly grew very pale, complained of faintness, and was caught while in the act of falling to the ground. He was carried

to the hospital department and given stimulants. The physician arrived in a few minutes and found him dying.

A post-mortem examination was made a few hours later by Dr. Robinson, assisted by Drs. Rudderow, Taylor, Weideman and Seltzer. A large tumor found just above the heart, proved to be an aneurism of the posterior ascending portion of the aorta. The sac contained $1\frac{1}{2}$ pints of freshly-clotted blood, unorganized, and its posterior wall was formed from three dorsal vertebræ, the bodies of which were very much eroded. There was also some erosion of the corresponding ribs, and a slight bulging of the sac between them. The heart was very small, but free from disease. No other organic trouble observed.

A careful review of the case suggests the following thoughts to me: 1. That the disease was one of long standing, and that on account of the enforced quiet life of the subject, no inconvenience was experienced until erosion of the vertebræ was so extensive as to cause neuralgias of the adjacent nerves, and slight interscapular pain of but short duration. 2. That the case was rendered still more obscure by the facts that the opening into the sac was large, and that considerable portion of the sac-wall was made up of bone—both of which tend to destroy the possibility of there being a diagnostic aneurismal bruit. 3. That if there had been much interscapular pain, due to the erosion of the vertebræ, and it had been treated by introducing a seaton or issue in its vicinity, as has been lately recommended by standard authority (Thomas Hayden—Quain's Med. Dict.), there would have been considerable danger of puncturing that portion of the sac protruding between the ribs. 4. That there can be a very large thoracic aortic aneurism without any bulging of the chest-walls, and that it may erode much of the bodies of the vertebræ without causing spinal curvature. 5. That as there was no hemorrhage into the thoracic cavity or canal of the spinal cord, death must have been due to pressure on some of the cardiac nerves or ganglia, and consequently collapse from heart-failure.

AN INTERESTING CASE OF FEMORAL HERNIA IN AN
AGED PATIENT—STRANGULATION—OPERATION,
WITH CURE.

Read September 19, 1883.

BY WM. R. D. BLACKWOOD, M. D.

Neurologist and Electrician to Presbyterian Hospital, Physician to St. Mary's Hospital.

MR. B., aged seventy-three, tall, thin, but bearing his years well, and with a previous history of good health, was taken ill with supposed cholera morbus, on Saturday, August 11, 1883, but from the suppression of alvine dejections shortly after the seizure, and the rapid change of the vomited matter to a manifest stercoraceous fluid by the time his family physician—Dr. John Ivison—saw him, the diagnosis of the family was set aside for one of more grave nature. The gastric distress was very great, the desire to vomit urgent, and the act occurred at short intervals. Previously to his illness the gentleman had been extremely constipated, and the amount of liquid and semi-solid fecal matter rejected per oram was simply inconceivable except to those who saw it. At each instance of vomiting, a fair-sized wash-basin was half filled, and the total quantity thrown up before relief ensued was, to speak within bounds, fully three gallons, showing the enormous distension of the intestine which may occur in chronic constipation.

The case was believed to be one of obstruction, with possible invagination of the bowel, and efforts were made toward overcoming the difficulty and obtaining an operation in a normal direction, but without success. The question of strangulation by hernia was not at first accepted, because, although there existed an evident swelling at Poupart's ligament, the size of it was so small as to be taken for an enlarged gland, the patient having had glandular enlargement previously. There was no local *pain*, no *tenderness*, no *tension*, no *dragging* sensation at the site of the swelling, no tympanites—in short, the usual local signs of strangulated hernia were absent. The little tumor also was movable to such an extent as to easily deceive one, even when closely examined. The doctor having asked my opinion as to the condition of the patient, we saw him together, and I coincided with him as to the difficulty in stating positively to the family the evidence

of hernial strangulation, but as the patient was rapidly sinking from exhaustion, we decided to make an exploratory incision and determine the nature of the enlargement, the justice of this being explained.

The patient was etherized, and after cutting down to the sac the mass was found to be an entero-epiplocele. The sac contained very little serum, and considering the fact that strangulation had doubtless existed for at least thirty-six hours, the bowel, though highly congested, was in good condition. The gut was tightly nipped at the crural ring and was nearly empty, which accounted for the small size of the tumor. The hernia was internal to the femoral vessels, but closely adherent to them. It was reduced with some difficulty and pushed far up to clear the deep thrust of the needle in closing the wound. The stitches, four in number, were deeply taken and the ring obliterated as nearly as possible. A compress of lint, wet with a mixture of Canada turpentine, oil of sweet almonds, carbolic acid and glycerine (an excellent dressing), was applied under a spica bandage of the groin, and the patient made comfortable in bed. He reacted well from the operation, vomited freely once thereafter, and in an hour and a half passed an alvine defecation without pain or difficulty. The appetite returned moderately at once, food was well retained, and his strength returned nicely. For forty-eight hours the wound did well, healing for one-half its length by first intention, but then inflammatory action set in and free suppuration ensued. In a week, however, the wound had closed, and the old gentleman was able to walk his room with comfort.

The diagnosis of femoral hernia is, at times, not easy, being, as here, liable to be confounded with enlarged glands, or localized varix of the saphena. There was no impulse on coughing, no effort at reduction affected it, and excepting only the very small tumor, all the local signs of strangulated gut were absent. The advanced age of the patient was against him, and his depressed vital power was a bad factor. The result shows the value of exploratory operation under the circumstances, and I believe that the attendant is unfaithful to his patient if he does not insist upon it in every suspicious case. The wound is of little account—it does not enhance the danger which is already imminent, and it may, as here, save a valuable life.

SCIRRHUS OF THE MAMMARY GLAND.

Read September 19, 1883.

BY WM. R. D. BLACKWOOD, M. D.

Neurologist and Electrician to Presbyterian Hospital, Physician to St. Mary's Hospital.

THE tumor which I exhibit to you this evening was removed from a lady this morning. The growth has only been noticed by herself for about seven months, but it has undoubtedly existed for a longer period. It is an *atrophying scirrhus* of the right mammary gland, is densely hard, and was all that remained of the breast, which was thoroughly extirpated, together with the underlying fascia, all discoverable lymphatics, and a portion of the pectoralis major muscle. The character of the growth renders her chances for complete recovery doubtful, as such varieties of cancer are recurrent in a high degree.

She reacted well from the operation, but had a moderate secondary hemorrhage four hours after I left her, which was controlled by pressure. I learned this evening that she had a decidedly hemorrhagic diathesis, which renders my peace of mind unpleasant just now, for as it never rains but it pours, I am worried by two cases three miles apart, which tend to keep me in hot water, which, though efficient as a hemostatic, is undesirable as a factor in personal comfort.

246 NORTH TWENTIETH STREET.

DISCUSSION ON CASES OF HERNIA AND SCIRRHUS.

Dr. Phelps: The interesting point in this case is the small size of the tumor. About ten years ago I had under my care a woman who was supposed to be simply constipated, and who had a tumor in each groin. One of these was an enlarged gland but the other was doubtful. Dr. W. H. Pancoast was called in consultation, and he cut down on the tumor and found behind the gland a small lump of intestine in a gangrenous condition. The patient died.

Dr. J. M. Barton remarked: The reader of the paper speaks of the advantages and the safety of an exploratory incision. I fully agree with him. The operation for hernia is in itself remarkably free from danger; this is well shown by the almost certain recovery of cases when operated upon early, and their almost invariably fatal termination when several days have elapsed before surgical aid is invoked. When we have decided symptoms of strangulation, and an uncertain mass in the situation of a

hernia, it is certainly the duty of the surgeon to make an exploratory incision, even though the mass should present the appearance of being glandular, for we may find among or beneath these glands, as I have done, a small knuckle of omentum from which the dangerous symptoms arise.

Dr. Collins: I feel much interested in hearing the justification of the exploratory incision. In one case in which I used it, I found only liquid, no intestine; the patient died. I am glad to hear that the method is justified by surgeons. I recall also the case of a lady who had a tumor in the groin accompanied by vomiting. On exploration no hernia was found. The patient died and *post mortem* showed death from large gall stone, which had escaped into the intestines, thus producing obstruction, from which death ensued.

THE DUTY OF THE HOUR.

BEING AN EXAMINATION OF THE RELATION OF THE MEDICAL PROFESSION
TO THE GENERAL USE OF ALCOHOLIC LIQUORS.

Read September 26, 1883.

BY HENRY LEFFMANN, M. D.

I N his work on the descent of man, Mr. Charles Darwin, of blessed memory, remarks that he made in the course of his studies a large collection of the definitions which have been offered as expressing the distinctions between man and the lower animals. The primary object of this collection was to show the insufficiency of such definitions, but unfortunately the learned author abandoned his plan and the list was never published. I have always regretted this because I was anxious to see if any one had been bold enough to sacrifice the honor of the race to its independence, in other words to define the human being as the only animal in which natural passions are abused and unnatural appetites developed. Though it may be a pessimistic view of human nature, yet we cannot avoid the conclusion that the definition is substantially correct. The history of races and nations presents us invariably with a picture of unbridled passions, the fierceness of which is but slowly and uncertainly assuaged by civilization, for in the modern as well as in the ancient world, it is in the centres of intellectual development that the greatest license has been seen. Legislators, both of the civil and ecclesiastical order, have wrestled with these problems, and in some forms of excess have tried every expedient, from the most desperate repression to the most indulgent remonstrance, but with only

partial advantage. Among the vices which appear to be characteristic of man under every climate and social condition, is the use of alcoholic liquors, and although the evils of this indulgence have been vividly presented to every one, yet a determined effort to obliterate the habit belongs only to our own time.

In that almost exhaustive treatise on moral and religious polity, the Jewish and Christian Scriptures, we notice that the duty of total abstinence has not been inculcated either among Hebrews, although the daily duties of life were regulated with microscopic minuteness, or among the leaders of the new dispensation, although they founded a most extended system of asceticism and self-denial. We are concerned, however, with the present, not with the past. Around us is a social system of great complexity. Though progress is slow, yet we need have no fear of its general direction. Each year marks too slight a movement to permit us to distinguish the result, but each century gives us a definitely recognizable advance, and shows clearly the tendency of the race to a higher and purer life. It is the text of my discourse to-night that the basis of this higher morality is self-restraint, and the basis of self-restraint is the influence of example. In the consideration of total abstinence, and the relation of the medical profession to its encouragement, we must clearly distinguish between the use of alcohol as a beverage and as medicine. With the question of its therapeutic indication and contra-indications, we have absolutely nothing to do in this paper. As to the method and form of its clinical use, however, as will be shown later, very important questions arise.

I think I may safely assume that the use of alcohol is not necessary to the maintenance of ordinary health. Its physiological effects have been extensively studied, and concordant results have not always been obtained. I need not stop to reconcile these differences, for the greater portion of the published results is not germane to my subject, nor will it be necessary to devote time to the presentation of statistics. One authority will be sufficient, because it is an authority in whom opportunities of observation and experiment are combined with sound common sense and accurate logic. Without desiring to slight the labors of other workers, I think we find in Parkes' Hygiene the whole subject of alcohol so thoroughly discussed as to render other authority superfluous. In this work it is established beyond

question that the use of alcohol is not beneficial, that it does not increase the power of the system to resist extremes of heat, cold or fatigue, and that even in special cases in which stimulants appear to be needed to maintain the resisting powers, other substances may advantageously be used. It is certainly surprising to read that one of the most common opinions, I would rather say superstitions, about alcohol, that it assists the body in resisting cold, is without foundation. Scarcely any of the minor causes of drinking are more general than this, yet the unanimous testimony of those who have been in charge of polar expeditions is against its beneficial action in such vicissitudes, and some of these leaders have after their first experiences declared that they would not take on any subsequent voyage any person addicted to the use of stimulants. As regards the general effect of the continual use of alcohol on persons in ordinary health I cannot do better than quote briefly from papers read by well-known clinicians, before this Society, two years ago. Dr. Wood says:* "Although I hold that the habitual use of alcohol is to well-fed persons not only unnecessary but positively harmful, it seems to me that in many cases of disease and in those periods of life when by reason of age the body waxes weak, alcohol is found of great value. Under sixty years of age the daily employment of wine may for most persons be very well discountenanced * * *. It is notorious that in America almost every one in reasonable health consumes much more food than the system needs, so that any alcohol taken is added to that which is already in excess." Dr. Pepper holds † that the quantity permissible is very small, not more than one and one-half ounces of absolute alcohol in twenty-four hours, taken much diluted and only at meals. A very large number of persons, either from susceptible stomach or a gouty diathesis, cannot safely take alcohol at all. Dr. Bartholow says:‡ "As a stomachic tonic alcohol is effective only in the case of those not habituated to its use * * *. That in time a catarrhal state of the mucous membrane is produced and a pathological secretion obtained shows the impropriety of the long continued use of alcohol as a stomachic tonic." Finally, although relating to the therapeutical use of

* Is Alcohol a Food. Proceedings Phila. Co. Med. Society, vol. III, p. 135.

† Effects of the prolonged use of Alcohol on the Nervous System and Organs of Special sense. *Op. cit.*, p. 139.

‡ Alcohol. Its therapeutical uses internally and externally. *Op. cit.*, p. 127.

alcohol, I cannot avoid quoting some forcible and logical remarks made by Dr. Woodbury* in a discussion on the treatment of pulmonary consumption: "Nothing in clinical medicine is more certain than that the continual use of alcohol in even moderate doses stimulates the development of connective tissue all over the body, nothing in pathology more evident than the fact that alcohol is a prolific source of pulmonary disease, nothing in toxicology better established than the observation of the action exerted by alcohol upon the respiratory centre. For this reason it is especially dangerous in pulmonary consumption."

It is, unfortunately, too true that no quotations from authority or rehearsal of statistics are needed to show the moral and physical injury done by alcohol. Directly and indirectly it is a prime factor in the promotion of disease and crime, and when we reflect upon the thousands of desolated homes and ruined prospects for which this agent is annually responsible, we cannot wonder at the sentiment which is slowly but surely developing in the community against all phases of industry or trade which have for their object the furtherance of the use of alcohol, nor can we doubt that to the success of the work of moral regeneration of our race the obliteration of these industries is essential. A powerful assistance in securing and maintaining sobriety would be to destroy the superstitious respect in which the various beverages are held. Non-medical persons are generally aware that physicians attribute particular values to particular liquors. In my own experience I have found very few persons who are willing to admit that they use liquor merely because they like it. They generally find some other reason—the necessities of the system, the advice of some physician, either to themselves or to some friend. One person uses beer because it is a tonic; another, because of its nutritious value, and so on—every reason but the real one, because they like it. Not a little of this popularity of liquor is due to the glamour of sentiment which attaches to it. Even the austere psalmist, who, with the exception of a single sin, "did that which is right in the sight of the Lord," has praised the "wine that maketh glad the heart of man." And for ages poets and prose-writers have extolled the qualities of stimulating beverages and the romance of their manufacture. In our time, however, these sentimental

* Proceedings Phila. Co. Med. Society, vol. iv, p. 175.

features are but imaginary. Nothing in the present methods of producing liquors is of a character to make us respect them as types of poetic or convivial relations. The wine that stands on our tables no longer shows in its ruddy color the rainbow tints—

“Caught where the morning sunbeams, stooping low,
Have kissed Grenada’s plain.”

Nor does its aroma repeat

“The dainty perfumes of the East
That Horace used to praise.”

No, the suggestions that are now called up by those who know the facts, are the suggestions of the fourth floor of a Front street warehouse, where rectified spirit, animal charcoal, glycerine, saponified cottonseed oil, aniline red, burnt sugar, *et hoc genus omne*, are being mixed together and transferred to casks and bottles ornamented by lying labels. The foaming tankard of malt liquor no longer suggests the

“— house where nut-brown draughts inspire,”

but the images now appropriate are those of bloated workmen, aloes, quassia, and the hop substitutes, salicylic and boric acid, baking-soda, gum for preserving froth, and beer-pumps for producing it. In short, no romance belongs to our alcoholic beverages. They are the products of influences allied with the lowest levels of mercantile honor, and their touch is corrupting.

In an article read before this Society two years ago,* I put forward the view, that when alcohol is to be used by physicians it should be used as such, and not in the form of special manufacturers. I cannot express myself better than by my words on this occasion, as follows:

“We know that liquors prepared by strictly natural methods are not constant in composition; we know that under the exigencies of trade additional conditions of variations are produced, and even complete substitution brought about. I have for some time thought that the best way to secure entire constancy in the therapeutic use of alcohol would be to have the preparations made up by regular prescription, or by printed formulæ in the Pharmacopœia. The substances which exist in wine, beer or brandy are

* Medical Relations of the Commercial Adulterations of Wines and Liquors. Proceedings Phila. Co. Med. Society, vol. iii, p. 132.

in accidental mixture—some are useful, others useless. Why should we not have the useful articles properly combined by competent hands, and the useless omitted. * * * And the physician, instead of ordering a special wine, will simply prescribe such preparations as may be necessary of alcohol, water, flavoring ethers, and astringent or bitter principles." These prescriptions, like those containing other powerful ingredients, should be renewable only at the instance of the physician.

I have lately learned with much pleasure that Dr. A. W. Miller, of this city, a gentleman well known to most of the members of the Society, as an experienced pharmacist, is about to publish a paper advocating a similar view. Dr. Miller indeed expressed such opinion publicly several years ago, although I was not aware of it then. His large experience in the manufacture of flavoring, coloring and other materials used in liquor imitation, gives him the right to speak with authority, and I find by my conversation with him that we are entirely in accord. In his paper he intends to call attention to the fact which I would not have time to consider, that, in wines and brandies, factitious articles are sold at high prices, and thus the practice of ordering such articles exposes patient to both deception and robbery. Not the least of the injuries which is done to the community by the laxity of physicians in reference to the use of liquors, is the encouragement which is thus given to the sale of quack medicines under the guise of bitters and tonics. No greater fraud is put upon the public than the preparations which are advertised under these names. They are alcoholic beverages in their most dangerous and insidious form. I have this week examined one of the most extensively advertised of the lot—Warner's Safe Tonic—and I find it to contain a considerable amount of alcohol, in association with some vile combination of syrup and bitter extract. When it is remembered that the miserable stuff is bought at a price much above its value, and is used mostly by persons already somewhat out of health, we must see that the harm done is incalculable. Yet the popularity of these articles is largely due to the fact that they meet what most people believe to be a necessity in disease, an alcoholic tonic. During the last few years several eminent physicians and chemists in this country and abroad, have gone almost into spasms over a knowledge of such adulterations as the use of alum in baking-powders, glucose in candy, and oleomargarine in butter, all trifling

and non-injurious substitutions; but we hear very little about the far more dangerous preparations of the class just alluded to. The most striking evidence of the profoundly misguided condition of the public mind on these topics, was well shown lately in New York, when the officers of the Business Men's Moderation Society gravely condemned the use of the harmless glucose in beer, and then gave, inferentially at least, certificates of wholesomeness to beer containing between four and five per cent. of alcohol. The quack medicine mentioned above has with each bottle the official certificate of the Professor of Chemistry of the University of Rochester, stating that the preparation is free from deleterious ingredients. I feel sure that statements like this could not be made if medical authorities were true to their own knowledge on these questions.

It is in view of the points which I have here enumerated, that I feel obliged to lay before this Society, and through its published proceedings before the world, the accusation that the medical profession is responsible for a very large portion of the misery which alcoholic beverages produce, and I declare that the time has now come when a stand should be taken in favor of abstinence. I believe that it is established by the citations I have given, that alcohol is not needed by healthy persons. I know that many non-medical persons use liquor because of the general approval of it by the medical profession, and I think it can be demonstrated that although alcohol itself is a substance of great value, alcoholic beverages are entirely unnecessary. Of late years although physicians have assumed the right to speak broadly upon many questions affecting public health and public morals, they have been singularly conservative as regards the evil of moderate drinking. Yet it seems to me that sewer construction, registry laws, quinine pills, river pollution, ethical innovations, etc., on which topics so much energy has been expended recently, do not approach in magnitude the reform which is here urged.

The pollution of a river water by organic matter before it reaches a city reservoir is rarely so serious in its effects as the pollution of it by alcohol after it leaves the hydrants, and the dangers of Rye Beach, of which we have heard so much, are trifling compared with the dangers of rye whisky or what is labeled as such.

The learned professions are potent influences in moral reform,

and for many centuries law and divinity have exercised much more control over the race than has medical authority. This relation is now rapidly changing. The questions of civilization are regarded as practical problems, largely medical in character, and the direction of education is passing into the control of the scientist and physician. Both the lawyer and divine have recognized alcohol as a foe to public and private virtue, for courts now frequently regard intoxication as an aggravation rather than as an excuse for crime, and the almost unanimous temper of churchmen is against any form of indulgence in stimulants; even the time-honored employment of wine in Communion is not sufficient to maintain its use, and unfermented wine is now a familiar article of commerce. Let us then begin at once to discharge our duties, and ally ourselves openly with the laity, who, though lacking in scientific knowledge, have the good of the community at heart. Let us recognize that while many evils claim our attention, the importance of a firm stand in favor of total abstinence is urgent and is indeed the "duty of the hour."

DISCUSSION ON THE RELATION OF THE MEDICAL PROFESSION TO THE
GENERAL USE OF ALCOHOLIC LIQUORS.

The President stated that the points presented for discussion were :

1. That the use of alcohol in any form and in any amount by persons in ordinary health is deleterious.
2. That the medical profession, by its lax attitude on this question, is responsible for much of the prevailing abuse of alcoholic liquors.
3. That if alcohol is to be used at all, it should be given as such, and the prescription should be made non-renewable, as with other powerful medicines.

Dr. J. T. Eskridge, in opening the discussion by request of the President, said : I like the practical and novel way in which the subject has been treated. It will attract attention, and, I hope, serve to make the members of this Society, and of the medical profession in general, consider their own responsibility for the abuse of alcohol.

One of the conclusions at which the writer of the paper has arrived, is "That the use of alcohol in any form and in any quantity by persons in ordinary health is deleterious." This is a broad and sweeping statement, and, while in the main it is correct, circumstances may arise when alcohol may be administered to persons in health with benefit.

Dr. Percy's experiments showed that the free use of alcohol tends to prevent the solidifiability of fibrin, and thus renders wounds difficult to heal ; and those of Vierordt and Prout, that less carbonic acid is given off

in the exhalations by the breath under similar conditions. Bouchardat first pointed out that alcohol darkens arterial blood. These results were obtained by experimenting upon subjects whose stomachs had been overcharged with alcohol, or into whose veins the spirit had been directly injected. They show the poisonous effects of alcohol, and reasoning from these, it by no means follows that the administration of small quantities well diluted is injurious to health. But observations are not wanting to prove the evil effects of alcohol on healthy persons when taken in small quantities for a considerable length of time.

Prof. W. B. Carpenter, a strong advocate of temperance, in his admirable prize essay on the "Use of Alcoholic Liquors in Health and Disease," after describing the baneful effects of alcohol on mind and body, when taken in small quantities several times daily for a length of time, frankly admits that its temporary administration to persons in health, on certain extraordinary occasions, is attended with decided benefit. Prof. Miller, of Glasgow, in an excellent review of the subject, entitled "Alcohol: Its Place and Power," arrives at conclusions almost identical with those reached by Prof. Carpenter.

I am satisfied that persons in health, under ordinary circumstances, do not need an alcoholic stimulus, and that if its administration is attended by no good results, evil only follows the use of alcohol at such times; but on the other hand, I am convinced that circumstances do arise when, if alcohol is properly administered to persons in health, its good effects far outweigh its evil; and that there are conditions, short of what we are accustomed to call disease, which are improved by the temporary and judicious employment of alcohol.

Bodily and mental labor that cannot be endured without resorting to artificial stimulants had better be left off; but times come in the history of many persons, when they are not their own task-masters. A great deal may depend upon a few hours' work. Tea and coffee may not be sufficient stimuli, and a little alcohol taken at these times will allow an extra strain being made upon the system. The use of alcohol must not be often repeated for the purpose of increasing the power of endurance. It must be remembered that a stimulus in these cases acts the part of the spur to the tired horse. It probably does not directly add force to the individual, but it enables him to call more upon his latent powers, and, of course, the exhaustion which follows is all the more profound by the reason of the extra strain upon the vital forces, made possible by the use of alcohol.

Again, the appetites of some persons have been rendered capricious by the process of coddling, and those of others lessened, and the power of digestion weakened by worry and over-mental exertion, so that the simplest articles of food cannot be digested properly. To such the administration of a little wine, with a bitter tonic, before meals, for a week or two, and subsequently a bitter tonic given before meals, and wine during or immediately after the ingestion of food, is followed by admirable results. In all these cases the use of alcohol must be cautious and temporary, and not

allowed to be continued longer than is absolutely necessary. I agree with the statement made by the reader of the paper, to-night, that below sixty years of age a person is generally not benefited by the use of alcohol in health. I know, indeed, of a case in which it did not become necessary to resort to it before the ninetieth year.

In regard to the use of alcohol in phthisis, to which a casual reference has been made, I have very strong convictions of its value when properly employed. In this disease, when pulse is rapid and temperature considerably elevated, alcohol is contra-indicated, but in the more chronic cases, when respiration is difficult, and pulse and temperature nearly normal, its beneficial effects in prolonging life are evident to every one who has given it a fair trial. Prof. Flint speaks in the highest terms of alcohol in phthisis.

As a preventive of phthisis, alcoholic stimuli have their place and power. It is well known that in the same family, several children, whose parents have suffered from phthisis, may die from this disease at about the same age. For these persons, if alcohol is occasionally employed judiciously, whenever vital force falls below its normal in them, I have no doubt that in many instances the fatal disease might be prevented.

To the second proposition I may say that I do not know of any case in which the use of alcohol in disease, under direction of a physician, gave rise to drunkenness. Some time ago Dr. Hamilton, in a discussion before this Society, mentioned a case in which a patient became a drunkard in consequence of the use of alcohol in typhoid fever, but it appeared on further inquiry that the man was a habitual drinker before the disease occurred. Lax prescribing, however, may easily become a serious error. It is important that if alcohol is ordered, the quantity, form and time of taking should be indicated. It should be taken only at meals. This periodic and formal use of it will make the patient willing to stop when required. I admit that if we consider that alcohol is injurious in health, we must regard its use in social gatherings, and especially in those composed of medical men, as wrong.

In reference to the last points, I cannot speak from experience. If the adulteration is as extensive as pointed out in the paper, then the author's view, that alcohol should be used as mentioned, is correct. I however, doubt that we can make, extemporaneously, mixtures which will take the place of natural liquors.

Dr. Mills said: I would not wish to be regarded as an advocate of anything else but temperance in the best use of the word, but the question of the influence of the moderate use of alcohol upon intellectuality and the longevity of intellectual workers, is one of considerable interest, from what might be termed a biographical point of view. During the International Medical Congress of 1876, one of the English delegates expressed the opinion, in words which I do not exactly recall, that the intellectual productions of men who did not use alcohol at all were not of a character to indicate the value of abstinence. Many distinguished men who have

lived to a comparatively advanced age—the English Lord Chancellors, German thinkers, and well-known American statesmen, for instance—used alcohol in moderation throughout their lives. I simply introduce this point for discussion. About the evils of the abuse of alcohol no doubt can exist.

Dr. Wood: I consider the first proposition advanced by the author of the paper to be untrue; it is entirely too sweeping. To say alcohol is deleterious in *any* form and in *any* quantity in health, is to say that one would be injured by simply smelling a bottle of whisky. I am fully convinced that we do not need alcohol in health, but indulgence in it moderately, on occasions, is probably no more hurtful than over-eating. I have seen, at social gatherings, total abstainers, who, while standing apart from the general company and congratulating themselves upon their superior virtue in not indulging in stimulants, gorge themselves beyond repletion with the food set before them, much to their stomachs' distress.

The moral question involved is the old one of use and abuse. Whether I must forego the use of a thing because some one else abuses it.

Are we to abstain from a certain amount of pleasurable indulgence because of the example which that indulgence offers to others? I do not believe in attempting to force total abstinence, because I do not believe that the movement will accomplish the desired result. Not long ago I was traveling in Kansas and I met a prominent prohibitionist, a member of the Central Committee of the State, whom I questioned about the success of the prohibition movement, asking, *inter alia*, if they had destroyed the grape industry, as the law directed, in the wine-making districts. He replied that they did not expect to do this in those places. Here was an admission of the weakness of the cause, for where the manufacture and use of wine were now most active was the least hope of abolishing it. In the same car was a traveling salesman on the verge of delirium tremens, and I asked him as to the effect of the liquor laws in Kansas. He replied, "I can get a drink of whisky anywhere in Kansas for fifteen cents." The Scandinavian method of dealing with the temperance question seems to me more practicable. In Sweden and Norway the country is divided into districts, in each one of which only one tavern is allowed. The licenses are sold at public auction. The temperance people have combined and bought up the licenses. They are obliged to open the tavern, but they can adopt such regulations as will prevent the excessive use of alcohol by those who frequent the place, and also employ all moral means to persuade men not to drink at all. I do not think that the adulterations of liquor are as harmful as has been stated by the lecturer. Liquors and wine are artificial products always.

I think that good liquors can be easily obtained. I do not agree with the proposition that medical men are responsible for the habit of drinking to excess; on the contrary, the example and teaching of the medical profession have done much to diminish the evils of intemperance.

Dr. O'Hara: In reference to the remarks of the last speaker, let me read the following from Richardson, "Induced Diseases of Modern Life," page

232: "Speaking honestly I cannot, by any arguments yet presented to me, admit the alcohols by any sign that should distinguish them from other chemical substances of the paralyzing narcotic class."

If this view be correct, we can have no doubt that alcohol is injurious in health, and that it does not serve as food. We are too much under the ideas of Liebig in this matter. For my part I cannot see that the total abstinence movement is a failure, or that the views advanced in the paper are erroneous. They are the views which have been advocated by high authority, in the International Medical Congress in 1876, and other scientific bodies, for instance. The medical profession may be responsible indirectly for much of the excessive drinking, through the idea scattered, but now passing away, that it was food.

I have myself learned by experience the evils of too much confidence in alcohol, when I thought it was food, and now watch it closely as a medicine. As to the cases narrated by Dr. Eskridge, they cannot be regarded as cases of healthy persons, and the use of alcohol in treatment of them is a question of therapeutics, not of hygiene.

Medical men may certainly accomplish a good deal by the teaching influence of example.

I recall an instance in which brandy was used for dyspepsia; the patient, it is true, got rid of the dyspepsia, but he complained frequently until the day of his death, which was superinduced by liquor, that he made a bad swap and would rather have held on to his dyspepsia.

The conclusions of Dr. Hunt's paper were adopted by the International Medical Congress, 1876, and ordered to be transmitted to the National Temperance Society, the Women's National Christian Temperance Union, and the Friends' Temperance Union of New York. They were:

1. Alcohol is not shown to have a definite food value by any of the methods of chemical analysis or physiological investigation.
2. Its use as a medicine is chiefly that of a cardiac stimulant, and often admits of substitution.
3. As a medicine, it is not well fitted for self-prescription by the laity, and the medical profession is not accountable for such administration or for the enormous evils arising therefrom.
4. The purity of alcoholic liquors is, in general, not as well assured as that of articles used for medicines should be. The various mixtures when used as medicines should have a definite and known composition, and should not be interchanged promiscuously.

Dr. Tyson: This question is one very difficult to discuss; both parties are apt to go to extremes. Dr. O'Hara's remarks are a case in point, for the injurious effects to which he alludes are the effects of the use of alcohol in excess and not in moderation. It must have been the experience of all practicing physicians to see many cases which are benefited by the moderate use of alcohol, especially at meals, while in many aged persons its use is very appropriate and even necessary. I am not prepared to deny altogether the correctness of the second proposition offered in the paper.

I think there may be some ground for it, yet I do not know a single case in which the recommendation of the use of alcohol in disease has resulted in establishing a habit of drinking. I recall a case in which a gentleman was advised by a non-medical friend to use whisky for dyspepsia. It was tried, and finding good results from it he continued using it in small amounts daily; the use was kept up until one day the patient found the bottle empty. He missed his usual dose so greatly that he was forced to realize that he had been drinking, and never used the liquor again. It may be laid down as a rule that it is not safe for physicians to advise the regular use of alcohol for dyspepsia; it may lead to a habitual use of stimulants. As to the third point, I think that in view of the fact that it is still possible to get pure wines, especially if we are satisfied with domestic wines, the flavor and other properties which made them more acceptable to the patient, justified their continued use; but I for one am willing to try the effect of pure alcohol properly diluted in cases where alcohol only is indicated.

Dr. Hamilton: Moderate drinking, it must be remembered, is very often the road to immoderate drinking, and therefore the physician, whose influence in this connection is paramount, should sedulously avoid the too frequent and too liberal use of liquor, especially in young subjects. In the low forms of fever, or in chronic, wasting disease, such for instance, as pulmonary consumption, to which allusion has been made, it is often of great advantage, and in the latter disease, where expectoration is profuse, but unaccompanied with much fever or difficulty in breathing, it may prolong life for an indefinite period. The custom of drinking in the wealthy and fashionable circles may still be said to prevail with tyrannic power, and in ordinary social reunions the same practice is common. The influence of wealth and fashion is dominant, and until some amelioration in this connection is manifest, no general temperance reformation need be looked for in the people at large.

The allusion to the adulteration of wines and stronger liquors was deservedly made, but it occurs, doubtless, much more frequently in regard to the finer and more costly than to the cheaper liquors, and the perfection to which this adulteration has attained is simply notorious.

Dr. James C. Wilson: I am much pleased to see this subject before the Society. It is a subject which ought to be agitated, because the agitation will bring out the truth. I think that the propositions rather overstate the case, and somewhat weaken the points advanced. Independently of law, church influence and local politics, a widespread, popular sentiment is developing in favor of temperance, but not of total abstinence. A feeling against excessive indulgence is growing steadily in the community. It is now considered "bad form" to drink to excess at social gatherings, and young men especially are much more restricted than formerly. The three-bottle men of earlier days are now unknown. It is, however, going to an extreme to put the proposition that the use of alcohol in any form, and to any extent, is deleterious. Many persons can use alcohol in moderation, and derive comfort from it without injury. I do not agree with the second

proposition. Respectable medical men are not lax in their attitude on this question, but are accustomed to caution their patients in regard to the dangers of the use of alcohol. Most of them direct the amount to be taken and fix a time for discontinuing the use, just as they do with other powerful drugs. As regards the suggestion to use alcohol alone, it appears to be open to some objections. It is not possible to imitate the different wines. No formula of the pharmacopoeia or prescription could produce the perfect mixture which we see in natural wines, which are often so specifically beneficial.

Dr. Frank Woodbury: I have been much interested while listening to the paper, and, in the main, sympathize with its teachings. I think that the propositions submitted for consideration were purposely framed so as to excite discussion, since they are not logical deductions from the paper, and indeed have not been presented as such. The alcohol question is a complex one - it is a great social and moral problem, as well as a scientific and medical one. The subject as presented this evening has at least three aspects: the use of alcohol in any quantity in health - this is a physiological question; its employment by physicians - a medical question; and the right of physicians to prescribe it - which is a moral question. This moral question is really the principal one of the paper, as is shown by its title, "The Duty of the Hour." With regard to the question as to its injurious effect when used in health in any quantity, I would ask, first, what is meant by a state of health? If a physiological definition is accepted, then no person living under the artificial conditions of civilization can be in a state of perfect health. Ordinary health is merely an approximation towards physiological health; and if the utility of alcohol is acknowledged in the condition of disease (which is merely any departure from the healthy standard), then the first proposition is answered by the lecturer himself in the negative. Then again, concerning the use of alcohol "in any quantity." I think it worth while to recall the fact that not only is it without injurious effect in very small doses, but that in reality the organism cannot escape from imbibing alcohol; it is omnipresent - it is in fresh bread and in ripe fruits, and even traces have been found in the air we breathe, provided there exist a certain amount of organic matter and the conditions of temperature and moisture necessary to fermentation. Even in the muscular tissue and urine of total abstainers, a substance is present chemically indistinguishable from alcohol. In large doses every one admits that is capable of destroying life by its own properties when taken into the system, and it is therefore a *poison*. Its use in much smaller doses, it must be admitted, may not prove incompatible with the enjoyment of long life and ordinary health; but in many cases its constant use directly induces disease and tends to shorten life. The fact that a substance is a poison, however, is not sufficient in itself to forbid its use in disease, provided that it be given in accordance with the teachings of science and experience. The physician with the longest experience of any present has just expressed his deliberate opinion that it is useful in low conditions of vitality and in slow convalescence.

With regard to the right of the physician to prescribe alcoholic liquors, the moral aspect of the subject, I hesitate to express an opinion, for fear of being misunderstood. I would suggest, at the outset, that in reality the treatment is not so entirely under the control of the physician as is implied by the question. Is it not the fact that it is the patient who employs the medical attendant, and if he is not treated in accordance with his ideas he becomes dissatisfied, loses confidence in his physician and engages another?

A rough illustration may be given: When a Chinaman falls sick, he, as a rule, will, if possible, secure the services of the kind of physician that his parents and friends approve of. A sick Indian in the same way prefers the treatment of his medicine man to that of the most scientific post physician. Is it not also true, that in more cultivated communities, the physician who is called upon in the hour of sickness is the one whose thoughts and prejudices best agree with those of his patients? Patients certainly should not be allowed to dictate in the details of treatment, but their unrelinquished right to approve or reject the general plan of treatment cannot be disputed. It may seem like a humiliating admission, but it is true that in a community where the taverns far exceed the bakeries, total abstinence physicians will have more opponents than clients.

I hope that nothing that I have said will convey the impression that I approve of the use of alcoholic liquors in disease, given merely with a view to gratify the patient; if ordered at all they should be given to obtain the physiological action of ethylic alcohol, which, on account of variation of strength and adulteration in ordinary liquors, may be best given in the form of dilute alcohol in order to secure both purity and exact dosage. This, I have learned, has been largely employed in Bellevue Hospital, where it was introduced by Dr. Gillette; by its use the medical effects of the drug are obtained, and the danger of encouraging the use of ordinary liquors is to a large extent obviated. In conclusion, I wish to express the opinion that the second proposition should be reversed, and it should read: "Physicians deserve great credit for inculcating more correct and scientific ideas with regard to the use of alcoholic liquors by the community." The great advance in the cause of temperance, in my opinion, is very largely due to the teachings of certain prominent physicians and the precepts and example of the great body of the profession.

Dr. Leffmann, in closing the discussion, said: I am dissatisfied with the direction the debate has taken, for too much time has been given to the discussion of the value of alcohol as a remedy, a point which I expressly excluded. While the propositions which I laid down are my convictions, and I believe they will all be ultimately recognized as true, yet I purposely worded them in an extreme form to make the debate more definite. No one, however, is justified in giving to the first proposition the strained meaning that Dr. Wood put upon it, namely, that smelling a bottle of whisky would be held as injurious. The language should be judged according to its intention. The remark quoted by Dr. Mills, in regard to

the literary abilities of total abstainers, is also unworthy of the dignity of the question and is not argument. Dr. Woodbury's suggestion that alcohol must be used because patients expect its use is surely not the true principle of medical practice. Patients are not to be the judge of anything in treatment. The success of homœopathy and kindred delusions is largely due to the erroneous and absurd view that patients may elect the system of medicine on which they are to be treated. Neither can one regard the fact of the occasional existence of alcohol or alcohol-like bodies in organic matter, in urine or in muscular tissue, or in fresh bread (which latter I doubt very much), as any argument for its use in health as a beverage. Several of the speakers have apparently regarded the paper as declaring the adulteration of liquors to be harmful, but this was not the ground taken. On the contrary, in two papers on alcohol previously read before this Society, and papers read elsewhere, I have pointed out that the adulterations are cheats rather than poisons. It is the uncertainty and deception to which I call attention.

Although objection has been made to the third proposition on the ground that alcohol alone or simply diluted will not answer, yet we have these objections well refuted by the fact just stated by Dr. Woodbury, that in one of the New York hospitals such a preparation is regularly and successfully used.

The second proposition has been misunderstood. It has been taken to mean that the use of alcohol in disease under medical advice has resulted in drunkenness, but this is not the meaning. It is the habit of moderate drinking in health that makes drunkards, and it is to the indifference of the medical profession to this habit that the second proposition relates.

The whole question, it seems to me, is a most important one. The terrible effects of alcohol are seen in all directions, and if the restriction of it is needed—and I do not see how any one can doubt that fact—such restriction must only come by active assistance of those who know the facts best. It will never do to temporize with vice. No method will answer with any form of crime or vice, except continuous, unrelenting opposition. It is a mistake to suppose that widespread and deep-rooted habits are unconquerable. Did time permit, it could be shown that traits of human character, as vicious and deep-rooted as the tendency to drinking, have been eradicated by persistent effort, and what has thus been done can be done again. I am fully of the opinion that the tavern finds its best support in the idea so general in the community that physicians consider the regular use of a little alcohol not injurious. When the medical profession is true to itself, and teaches that alcohol should never be used except for a specific purpose and under continued medical advice, the tavern will lose its best hold on the community.

The statement of Dr. Wood that he is fully convinced that we do not need alcohol in health, and that indulgence in it moderately on occasion is no more hurtful than over-eating, certainly leans in favor of my first proposition.

NAPHTHOL—ITS MEDICINAL USES AND VALUE.

Read October 17, 1888.

BY JOHN V. SHOEMAKER, A. M., M. D.

Physician to the Philadelphia Hospital for Skin Diseases, Lecturer and Instructor on Diseases of the Skin in the Summer School and Post-Graduate Course of Jefferson Medical College.

NAPHTHOL is one of the remedies of recent introduction, and of the two products of that name the β naphthol is the one which was first used by Prof. Kaposi as a substitute for the tar preparations in skin diseases. It was thought by him as the essential and curative ingredient of tar, while it was free from the objectionable features of the latter.

My attention was directed to this remedial agent by Dr. Justus Wolff, a chemist largely interested in the manufacture of coal-tar products, who kindly furnished me a paper on the chemistry of this substance, along with some novel properties which he had observed in it. As this paper, however, is too long for reproduction here in its entirety, and besides is largely of chemical interest only, I will here give it briefly in abstract as far as will be necessary to acquaint us with the chemical character of its subject, as follows:

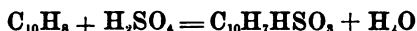
Naphthol is a derivative of naphthalene, a hydrocarbon found in large quantities in coal-tar, belonging to the so-called aromatic group. In the fractional distillation of coal-tar, various hydrocarbons are obtained at different degrees of heat. Thus at 80° C., benzol distils over; between 80° and 110° C., benzol and toluol mixed; at 111° C., toluol alone; from 111° to 136° , toluol and the different xylenes mixed; from 136° C. to 142° C., xylenes only; then the cumenes, phenol and cresols; and at 218° C., naphthalene, which sublimes in colorless, transparent, brilliant, crystalline plates, possessed of a disagreeable pungent odor; the empirical formula of which is $C_{10}H_8$.

Naphthol is produced from this by a substitution of one of the hydrogens in naphthalene by one molecule of hydroxyl HO.

According to the different positions of the hydrogen substituted in the naphthalene by the hydroxyl, two different naphthols are obtained, of which one is called α naphthol, and the other, the one we shall alone speak of hereafter, is the β naphthol of the formula, $C_{10}H_7HO$.

The naphthols demonstrate the advantage of a knowledge of the relative and absolute positions of substitution in order to understand the cause and constitution of the different offsprings from simple or compound constitutions.

The method of producing naphthol is like the general process employed in effecting hydroxyl substitutions by first producing monosulpho-substitutions, by means of strong sulphuric acid at certain temperatures, and melting the monosulphonated compound with sodium hydrate, the ordinary dry caustic soda. In the case of naphthalene treated thus with sulphuric acid, the naphthalenemonosulphonic acid is produced according to the following formula :



which on being melted with sodium hydrate, yields naphthyl hydroxide or naphthol, as per formula herewith :



According to the different temperatures employed in the sulphonation of the naphthalene, either α or β naphthol is derived by the last process. The naphthols thus produced are usually purified by distillation and brought in the market as crystalline masses of a reddish color, and a disagreeable and pungent odor, as shown in the specimen herewith submitted.

The β naphthol crystallizes in scale-like clinorhomboidic lamina from watery solutions, whilst from a molten state it separates as clinorhomboidic prisms. It dissolves in 520 parts of water at 60°F ., and in 75 parts of boiling water. It is readily soluble in alcohol, ether and chloroform. An aqueous solution is colored yellow by chloride of lime, and by heating this solution yellow flakes separate. It melts at 122°C . (Schaeffer), but a mixture of both α and β naphthol melts at a lower temperature than either alone. Compounds with alkaline metals or ammonia and alkaline earths are not stable, and separate easily, either by evaporation or in contact with carbonic acid.

The naphthols stand in the same relation to naphthalene as phenol to benzol and cresols to toluol. If one of the six hydrogens in benzol is substituted by hydroxyl, phenol is obtained ; in the same way are cresols and naphthols formed. By this analogy of constitution of naphthols, phenol and cresols, the inference

may easily be arrived at, that they may prove alike in their disinfectant character as well, and in order to prove this I undertook a series of experiments. Of course the commercial naphthol to that purpose was out of question, and I experimented, therefore, first to obtain a naphthol free from odor. As the crude article contains, as contaminations, sulphur and sulphurous acid, the sublimates thereof will yield, besides the naphthol crystals, also sulphureted hydrogen, thionaphtholes, carbolic and cresylic acid, thiophenols and the like, to which ordinary naphthol owes its pungent and disagreeable odor. I avoided this all by passing a rapid current of steam through its aqueous solution, expelling thus all volatile by-products, and obtained naphthol thus in its greatest state of purity, in beautiful silver crystalline scales, as here submitted. This naphthol may again be sublimed and obtained then in elegant white crystals as here shown, but by the heat employed more or less decomposition again takes place and renders the product somewhat odorous and pungent.

In order to test the disinfectant and antiseptic properties of my inodorous naphthol, I added one part thereof in powder form to 480 parts of urine, which at the expiration of six months, at a varying summer temperature, manifests no odor or signs of decomposition, while another of the same urine without addition of naphthol had a strong putrid odor already, after standing for three days only. To this latter I added, after standing thus for eight days, some of my inodorous powdered naphthol in the above-mentioned proportion, and in twenty-eight hours it had lost its putrid odor and has kept thus up to the present writing, when no putrefaction or signs of it can be detected in either specimen. The same experience I have made with meat immersed in a solution of naphthol in 520 parts of water, as well in other experiments similarly conducted.

Experiments with solutions of the compounds of naphthols with alkalies or alkaline earths prove that these act very much less antiseptic than the solutions of pure naphthol; soaps, containing four to ten per cent. of free naphthol, were found excellent and serviceable in removing odors of putrefaction or decomposition from hands or cloths. They are also very efficacious in destroying clothes- or body-lice, as naphthol is a very active parasiticide. If naphthol is evaporated by means of heat, the air in rooms contaminated in consequence of disease or otherwise, will be found to be rapidly deodorized and rendered fresh and sweet without

other odors, making it thus of the greatest value for sick-rooms, hospital wards, dissecting-rooms, etc.

As carbolic acid has many disadvantages, and is not the deodorant or antiseptic *par excellence*, the inodorous naphthol can certainly take its place in every respect. As naphthol has been described variously as poisonous and injurious to the animal economy, which by its composition and analogy was not apparent, I felt it my duty to experiment with it in regard to such, and commenced at once, without hesitation, by taking it internally; one part dissolved in 3000 parts of water produced at first heart-burn, a slight sensation in the right lumbar region, and some dizziness. Of that solution an equivalent amount was taken to represent a half-grain.

These symptoms disappeared after continuing its use for some days, and while the urine showed upon analysis traces of naphthol and naphthol compounds, no albumen or blood could be detected therein. The doses then were gradually increased to four grains per day for six days, and still no untoward symptoms were discovered, while the warmth in the stomach directly after taking, was followed by increased appetite. Dr. Schofield, of Albany, reports to me that upon my solicitation he has used it largely, at first experimentally in the Albany hospital, where it has now become a staple article, and is used almost entirely to the exclusion of other disinfectants and antiseptics. They use it there for all kinds of disinfection in wards, sick-rooms, for wounds, etc., and have abandoned carbolic acid in all but a few cases, and always with the greatest satisfaction and success.

The experience of Dr. Wolff, as just recorded, as well as that of Kaposi and others, led me, about nine months ago, to employ it in private and hospital practice, and the success attained led to further experiments. I found it to fully sustain the claim that Kaposi had made for it in scabies, psoriasis and chromophytosis, as well as in some of the chronic forms of eczema, in which it not only allayed the itching attendant thereto, but lessened the infiltration as well. In wounds and indolent ulcers I have found it a most useful detergent and deodorant, removing the fetor and establishing healthy action of the parts. Aqueous solutions, containing half-grain to the ounce, I have used to great advantage as vaginal injections, especially in leucorrhœa and uterine carcinoma.

as well as in gonorrhœal affections, both in male and female. In diphtheritic throat affections it made a most useful gargle, as well as to remove the fetor of catarrhal and other affections of the buccal cavity. Its greatest value, however, arose from its disinfectant action of the evacuation of fever patients and rooms containing them, while by its absence of odor it did not tend to produce inconvenience both to patient and attendants. Combined with powdered talc or starch, or both, and dusted into the shoes or stockings of those affected with fetid exhalations of the feet, it acts most satisfactorily, and its effects are equally as good in the same affection involving the hands, axillary and inguinal regions. Combined with other ointments in the proportion of from one to ten grains to the ounce, it not alone preserves the unguent from decomposition, but exercises also an antiseptic action to the parts and the exudation therefrom. A slight admixture to an experimental sample of lard has preserved the same in excellent condition throughout the hot summer months. In chronic psoriasis, particularly when there is great infiltration, a five to fifteen per cent. ointment has frequently been attended with good results. It has also been very effective in squamous and fissured eczema, used in combination with lard or gelatin.

To test for myself its antiseptic properties in comparison to that of carbolic acid, I mixed two whites of an egg with equal weight of water and took one-half of this mixture in one vial, adding one grain of crystallized carbolic acid, while to the other half in another vial I added one grain of Dr. Wolff's odorless naphthol. After the expiration of five days the carbolized albumen assumed a putrid odor, whereas the naphtholized part, though discolored by the naphthol, remains to this day, twenty days after the experiment, without odor. A quantity (about half-pound) of meat already commencing to putrefy, was also at the same date immersed in a saturated aqueous solution of naphthol, with the effect of arresting the putrefaction and preserving it for some time.

After using naphthol so long and successfully without any untoward occurrences, I read to my astonishment and alarm that Dr. A. Neisser, in the "Centralblatt für die Medizinischen Wissenschaften," 1881, No. 30, reported most extraordinary toxic effects obtained with naphthol, and that also Kaposi reported having seen hæmaturia, ischuria, vomiting, unconsciousness, and

eclamptic attacks in a boy after the external application of naphthol. Also that Squire reports of it in the "British Medical Journal," January 14, 1882, as producing blisters and irritating the skin.

Dr. Piffard regards it as a dangerous remedy, and Prof. Rapon, while he reports good results with it ("British Medical Journal," p. 750) in scabies, prurigo, and eczema, advises in prolonged cases simple ointment to be substituted every fourth week, to avoid any possible risk of absorption.

Dr. Neisser stated that one gramme of a saturated solution (which in water would contain about $\frac{1}{10}$ grain of naphthol) injected hypodermically in a dog produced hæmaglobinuria, and shortly afterwards death.

To verify these accounts and satisfy myself on the toxic effect of pure naphthol, if any it possessed, I administered to one rabbit, repeatedly in twenty-four hours, thirty-four minims of a saturated aqueous solution, hypodermically, without any result, either to inconvenience the animal, increase his temperature, diminish his appetite, or cause letal effect. This method of treatment was pursued for five days, not less than four to five injections being made per day, and the result was still the same. Determined to obtain toxic effects with it, and, if possible, to demonstrate its toxic action by a post-mortem examination, another rabbit was fed, at first, every three hours with one-grain pills of naphthol, and subsequently with two- and four-grain pills, at the same intervals, but, beyond increasing the appetite of the animal, no special effects were apparent. In consideration of this, one of my assistants, Dr. Charles S. Means, and my student, Mr. F. C. Waterman, volunteered to take naphthol themselves internally, to test, if possible, its action upon the human organism. They commenced with one-quarter of a grain dose every two hours—their pulse, temperature and urine being subjected to the closest inspection both before and after. The second day they took a half-grain every two hours; the third, one grain every three hours; the same on the fourth, while on the fifth and sixth they took two grains every three hours, and on the seventh five grains twice daily. The pulse and temperature did not appear to be affected by this, nor was at any time albumen or blood apparent in the urine. Though they experienced great warmth in the epigastric region after each dose, that passed away in a short

time, but left them with slight vertigo, buzzing of the ears, with all evidence of cerebral hyperæmia. The alvine evacuations were softened and of mushy consistence, changed to a clay-color, and in one of the cases increased to diarrhœa.

Arriving at a *résumé* of my experiments, I must certainly proclaim the odorless naphthol which I had received from Dr. Wolff as not a toxic agent; and while I have found it a most useful remedial substance, and a disinfectant and antiseptic of the greatest value, it does not, in my experience, confirm the dangerous influence exercised on the human organism as reported by the gentlemen above quoted; a fact for which I can only account by the greater purity of the material used by me—purified from the deleterious contaminations above enumerated by the process already described, which is not employed abroad, where yet naphthol is sold and used as reddish crystalline masses, with strong, pungent and disagreeable odor. That it is far superior to carbolic acid and other disinfectants and antiseptics I have no doubt, and I am informed that in price it is not alone cheaper than the former, but, by its greater efficacy and smaller amount necessary, it is certainly more advantageous, aside from its greatest recommendation of being almost absolutely odorless. It must be borne in mind that all my remarks apply to odorless naphthol—only such as I have exhibited—and that I consider that alone as safe for medicinal use.

DISCUSSION ON USE OF NAPHTHOL.

Dr. Van Harlingen : I have used naphthol largely, and have recently recorded my experience. I agree with Dr. Shoemaker as to its value in one or two diseases. I have tried it in seven different forms of skin disease. In scabies it is the best remedy we have. It is equally efficacious with sulphur in killing the itch insect, and it cures the accompanying eczema at the same time. In psoriasis it is not as good as some of the older remedies. It has failed in my hands in eczema, and also in hyperdrosis.

THE TREATMENT OF PSORIASIS.

Read October 17, 1883.

BY ARTHUR VAN HARLINGEN, M. D.

PSORIASIS is one of the commoner skin diseases met with in this country. The statistics of the American Dermatological Association show that it occurs in the proportion of about six per cent. in all diseases of the skin encountered. Daily experience would seem to indicate a still more frequent occurrence, because the affection is a disfiguring and annoying one, and therefore patients are more inclined to seek relief, and also because it is a stubborn disease and greatly prone to relapse. The history of a single case will often extend over many years, and bring it under the observation of a number of different physicians.

It is because of the comparative frequency with which psoriasis is met and its stubbornness to treatment, that I have selected it as the subject of my remarks this evening. Having had a good deal of experience in the treatment of the commoner forms of the disease, it is my intention to confine myself chiefly to the consideration of such remedies as have come under my own observation and use, only touching incidentally on others.

The object of treatment in psoriasis is the removal of the eruption as it exists upon the skin. We cannot hope with any degree of certainty in any given case to prevent a recurrence of the disease, or, if you please, a relapse. For the drug has not yet been discovered which will surely take away all tendency to the recurrence of psoriasis, and whoever promises a cure, in the wider sense of the word, to his patient, will, in a very great number of cases, find that he has been too sanguine. Fortunately, however, a certain number of patients do seem to recover. I do not know what has been the experience of others in this respect, but I have patients who have been under observation three, five, even eight and ten years without relapse. Such cases are, unfortunately, few.

Preëminent among the internal remedies which are useful in the treatment of psoriasis, is arsenic, which may be justly called a specific in this disease. I think I am not asserting too much when I say that eight out of ten cases of psoriasis of average character and severity, shall do better under the use of arsenic

than with any other remedy. I prefer Fowler's solution given in the average dose of four minims thrice daily. I think this solution is often prescribed in too large doses, and I am sure the dose of five to ten minims, as given in the books, is too large for ordinary use. Most patients bear a four minim dose very well, but there are idiosyncrasies. I have sometimes been obliged to limit the dose at the beginning to one minim in cases, where subsequently such toleration has been established that twelve minims have been taken with impunity. However, four minims is a good dose to begin with, and if the effect does not begin to show itself within ten days or two weeks, the amount may be gradually increased. Fowler's solution should never be given to the patient in a vial with directions to drop out the requisite number of drops. The patient is apt to make a mistake, vials of different sizes may pour out more or less in each drop, and there is always danger in leaving a half-empty vial of poison about the house. The solution is better given mixed with water, or with wine of iron or other convenient vehicle.

The effect produced by arsenic upon the eruption of psoriasis is, first, in diminishing the quantity of epidermic scales thrown off, and then in preventing the appearance of new lesions. The patches gradually lose their scaliness, begin to heal in the middle and disappear little by little. It must be remembered, however, that arsenic is a slowly acting remedy, and its use should be continued through many months to get the best security against relapse.

The other liquid preparations of arsenic used in psoriasis are Pearson's solution of the arseniate of sodium, and Donovan's solution of the iodide of mercury and arsenic. I have used the former in a few cases without noticing any perceptible difference as regards efficiency between it and Fowler's solution. The solution of mercury and arsenic (Donovan's), I have employed in certain stubborn cases with good effect where Fowler's solution has seemed to fail. The existence of syphilis as the cause of the eruption in these cases having been excluded, I am at a loss to account for the apparently greater efficacy of the mixed treatment. The dose given was as much as ten drops, and although this solution is weaker in arsenic than Fowler's, yet I am inclined to the opinion that the conjoint administration of the two drugs, mercury and arsenic, was the cause of the good result rather than the

increased dose. I should be inclined to use Donovan's solution in cases where Fowler's solution shall have failed.

The mixture of arsenious acid, black pepper and sugar of milk, known as Asiatic powder, and recently placed in the Pharmacopœia with the pepper left out, among the triturations, is of no particular value above the other preparations, and is not so convenient of administration.

Hypodermic injections of solutions of arsenic have been employed in the treatment of psoriasis, but I have had no experience in their use.

Next in value to arsenic in the treatment of psoriasis is iron. I commonly employ the tincture of iron in cases where arsenic does not seem indicated. There is one type of psoriasis which includes robust, rosy, well-nourished individuals, "the very picture of health." Such people have never been sick a day in their lives, or perhaps may have had slight attacks of articular rheumatism. Such patients do well under arsenic.

But there is another type in which the individual is thin, poorly nourished and perhaps somewhat anæmic. These are the cases which do well under iron, which is best administered in the form of the tincture of the chloride. With these two remedies, arsenic and iron, I usually succeed in curing ordinary cases of psoriasis, adding in rare cases cod-liver oil to the use of the tincture of iron when debility is present. Of course local applications are employed at the same time. Of these I shall speak presently.

In addition to the internal remedies mentioned, quite a host of others have been employed from time to time. Such are tar, carbolic acid, copaiba, phosphorus, tincture of cantharides, tincture of maize, carbonate of ammonia, acetate of potassium and other diuretics; the alkalies, as liquor potassa and the alkaline mineral waters. Of these I have found alkalies and diuretics useful in cases when a markedly inflammatory condition of the skin has existed. The other remedies I have not employed, nor do I think the reports of their usefulness based on a sufficient number of facts, except in the case of tar possibly, to make it worth while to try them.

Equally important with the internal treatment of psoriasis is the external management of the disease. It is, of course, desirable to remove the eruption as soon as possible wherever it may be situated; but when it is found upon the face, there is every reason

to endeavor its cure by all means and in the shortest time. External and internal treatment should therefore be combined when practicable. The first thing to do is to remove the scales. This may be done by means of local or general baths, wet dressings, etc., or by inunctions with fats and oils, by the use of soap, or by the action of impermeable dressings of india-rubber or oil silk. When only a few lesions are to be acted upon, a solution of salicylic acid in alcohol, 1 part to 16, well rubbed in with a sponge, will remove the scales very nicely.

The scales having been removed, the next thing is to apply such substances to the diseased patches as may most quickly modify the abnormal condition of the skin, and bring it back again to a healthy condition.

An innumerable number of applications have been recommended for this purpose, the most of which I shall pass over with only a mention. Such have been soaps and alkalies, citric and hydrochloric acids, sulphur, iodine and mercury, alone and in combination, together with the various forms and preparations of tar, creasote and carbolic acid.

All of these remedies have their uses, and most of them, especially the tarry preparations, I have employed time and again in years gone by and with moderate satisfaction. The introduction, however, of chrysarobin or chrysophanic acid some six or seven years ago, put quite a new face on the local treatment of psoriasis; and since then, with the aid of this and other later discoveries, we are able to work a much more rapid change in the appearance and condition of the skin in this disease.

As chrysarobin is perfectly well known to all here present, both as to its advantages and defects, I shall say but little about it in the ordinary form of its application, namely, as an ointment. When it first came out I tried it quite extensively, but its disadvantages seemed so great that I had already begun to restrict its use greatly in my practice, when a new agent appeared, which for every-day use has in my hands, until very recently, almost entirely superseded all other local applications. I refer to pyrogalllic acid.

I do not think pyrogalllic acid is by any means so well known as an application for the relief of psoriasis as is chrysarobin. If I may judge by the infrequency with which its virtues are mentioned in the journals (although I believe all recent text-books speak of it), it is not in general use. But it is, in my opinion,

one of the very best remedies we have for the cure of cases of psoriasis of average severity. Employed in the form of ointment, of the strength of one-half to one drachm of the pyrogallie acid to one ounce of simple ointment, the effect produced by it is almost as rapid and decided as that brought about by chrysarobin, without the accompanying discoloration. A slight blackish staining is all that is produced, and the ointment can even be employed in the scalp without markedly discolorizing the hair, if applied cautiously. It is desirable, however, not to apply soap or alkalies at the same time, because this causes a more permanent and deeper stain.

Pyrogallie acid can not be used in extensively generalized psoriasis, when large surfaces are affected by the disease, without a certain amount of danger from absorption, as indicated by strangury and olive-green or tar-colored urinary secretions. With care, however, and the occasional suspension of the remedy for short periods, I believe this remedy could be employed even in universal psoriasis with good effect.

One more external application in psoriasis remains to be spoken of—namely, naphthol. This drug, a derivative of coal-tar, was introduced into use several years ago by Kaposi, of Vienna, as a sort of substitute for carbolic acid. He recommends it very highly in psoriasis, in the form of ointment, about eighty grains to the ounce, and I have used it in this and other strengths, and also in solutions in alcohol and oil, with fairly good effect.*

Naphthol is not so active in its effect as chrysarobin or pyrogallie acid, but it is much more agreeable and is, I think, peculiarly well adapted for employment upon such parts as are exposed to the view, as the face and hands. Like pyrogallie acid, it must be used with caution over large surfaces, as absorption with toxic effects may be produced.

It remains to mention briefly two or three methods of application of these remedies which have recently been brought forward. The first of these is the treatment by medicated gelatine which was introduced by Prof. Pick, the well-known dermatologist of

* In a communication read before the American Dermatological Association, last month, and published in the *American Journal of the Medical Sciences* for October, I have given the results of my experience in the use of naphthol in various diseases of the skin, psoriasis among the number. I may refer to that paper for further details as to the action of the drug in this disease.

Prague. My attention was first drawn to this by a pamphlet which Prof. Pick kindly sent me in which he gave an account of his earlier experiments with medicated gelatines, but I have not as yet had an opportunity of testing this method of medication as I should desire. I may say, however, that the method does not seem to me calculated to prove convenient and popular in private practice. I had for some time been making some experiments in my service at the Polyclinic in the preparation of gelatines impregnated with chrysarobin and pyrogallic acid, but without much satisfaction, when Dr. Chas. L. Mitchell, the well-known pharmacist of this city, sent me some excellent preparations of his own, which seem to be perfectly adapted to the purpose for which they are intended. A bit of one of these gelatine sticks is cut off and placed in a water-bath, where it soon melts into a clear homogeneous fluid, which may then be applied to the lesions of the skin by means of a paint-brush. The advantages claimed are cleanliness and transparency. The coating of gelatine does not rub off on the clothes, and is therefore not so dirty as an unctuous application. A fresh coating can be painted on every day or two as the old layer wears off. The chief disadvantage of this method of treatment is that it requires apparatus which is not convenient to carry about, nor can the patient be trusted to employ it at his discretion. My own experience is that in psoriasis, at least, the gelatine applications are not active enough. I have not, however, used them with sufficient frequency to pronounce a positive opinion.

Recently a solution of chrysarobin in collodion has been recommended in the treatment of psoriasis by Dr. George H. Fox, of New York, and several dermatologists have confirmed his statements with regard to its efficacy. I have employed this preparation in one or two instances, but it has seemed to me so much less active than the chrysarobin ointment that I have not been encouraged to continue its use. It has one great advantage over the gelatine applications, however, and that is that it can be applied extemporaneously and without the paraphernalia which must accompany the use of the gelatine.

A few weeks since a pamphlet by Prof. Auspitz, of Vienna, reached me, in which that distinguished dermatologist recommended liquor gutta-perchæ as a vehicle for the application of chrysarobin. I at once obtained a ten per cent. solution, or rather

emulsion, of chrysarobin in liquor gutta-perchæ, and happening to have a case of psoriasis of the face and scalp under treatment, I gave some to the patient to apply once daily. The effect was so happy as to encourage me very much to hope that we have in this preparation the most convenient method of applying chrysarobin yet devised; and as chrysarobin is, after all, the most efficient local agent in the treatment of psoriasis as yet brought forward, I have no hesitation in urging the trial of this preparation on any one who may have a case of psoriasis under treatment. It is to be noted, however, that the same watch must be kept upon the skin for fear of exciting dermatitis as when the ointment is used. Only when the liquor gutta-perchæ dries, which it does very quickly, there is little or no danger of rubbing the chrysarobin over the good skin, nor is there much danger, if any, of staining the clothing.

DISCUSSION ON THE TREATMENT OF PSORIASIS.

Dr. Shoemaker: Dr. Van Harlingen has included in his interesting and instructive paper, about all the remedies which are used in this disease. Believing that psoriasis is due to an accumulation in the blood of an excess of certain excrementitious substances—a condition known as one of sub-oxidation, I always begin and continue the treatment with an object to overcome this peculiar state of the system. I accomplish this purpose by using such remedies as will act effectively upon the liver and kidneys. I rely more upon removing the excrementitious substances from the blood by these organs, as well as by making the skin very active by various baths, than by giving arsenic and other preparations for their systemic effect. I do not believe in arsenic, or any other remedy, as a specific for psoriasis, and believe they only act at times by assisting to overcome this peculiar state of the blood. Napier, of Glasgow, extolled, about a year ago, chrysarobin (known formerly as chrysophanic acid), given internally in one-half grain doses, as a remedy for psoriasis. Its action is that of a purgative pushed to toleration, and it will affect the blood and pale the skin of psoriasis patients just as any other purgative would, given under similar circumstances.

In using arsenic, which I often do, as an assistance to the treatment just referred to, I always prefer arsenic in the form of arsenious acid, or sodium arsenite. The great objections to arsenic solutions, especially Fowler's, are their unstable state, and the improper manner in which they are often prepared. I therefore seldom use them, unless they are fresh and well prepared at the time of administration. If such solutions are kept any length of time, they will undergo a change. I have not only made these

observations, but have seen the same referred to, in several medical journals, by well-known authorities. I hold in my hand an extract taken from the *Journal de Pharmacie et Chemie*, in which M. Delehayé refers to the fungoid formation in Fowler's solution. Here is another extract, taken from Lewin's "Accidental Effects of Drugs," in which the author states that it "has been proven that Fowler's solution loses arsenious acid in the course of time, probably under the influence of organic substances which have gained access to it. The acid is reduced, and escapes as arseniuretted hydrogen gas. Great loss may be occasioned in this way."

As to the manner of administering arsenic for the treatment of psoriasis, I always prefer, when I can, to give it by the hypodermic method. I generally use pellets of sodium arsenite, such as I exhibit, divided in one-sixteenth, one-tenth, one-fourth, and one-half grain doses, as manufactured by Dr. L. Wolff, of this city. Prof. Bartholow, in his recent work on "Hypodermatic Medication," speaks especially of the utility of this salt of arsenic subcutaneously, on account of it being a higher oxide than the potassium arsenite, and therefore it is less an irritant. I usually select the inferior scapular or sacral region for the injection, and repeat the operation every day, until the eruption shows some signs of abating. In the meantime, the constitutional and local treatment already referred to is continued. This method is precise, saves the alimentary canal from the irritant action of the drug, and acts in a safer and quicker manner than all other means of administering arsenic for a systemic effect.

I perfectly agree with all that Dr. Van Harlingen has said of the local treatment of psoriasis, except in the use of pyrogalllic acid and naphthol. I regard pyrogalllic acid as a dangerous remedy, having seen, in several instances, very unpleasant systemic results follow its use. Binner reported, in 1880, four cases of poisoning from the external application of pyrogalllic acid, in which two of them terminated fatally.

As to naphthol, no precaution whatever need be used in applying such as I have exhibited to you this evening. I have used it all over the surface of the body, without any untoward effect, both incorporated in lard and in gelatine. You have seen me spread this naphthol gelatine dressing on the typical case of psoriasis, which I exhibited to you in my paper on naphthol. The dressing is easily prepared with an ordinary tin or china cup suspended in boiling water. The operator can add ten or thirty grains of naphthol to one-half ounce of gelatine, with a little glycerine; stir the mass in the cup now suspended in boiling water and as the heat liquefies, the gelatine can be easily spread over the patches. Plenty of hot water in a few days will remove this dressing, that can be hastily and well applied in any physician's office or at any patient's home.

NOTE ON HYDRARGYRUM FORMAMIDATUM.

Read October 17, 1888.

BY JAMES C. WILSON, M. D.

SOME accounts of this preparation have appeared in recent journals. Towards the close of last year, Prof. Liebreich proposed, in a meeting of the Berlin Medical Society, a new drug for the treatment of syphilis by the hypodermic method. Chemically this drug belongs to the amide group. Liebreich was led to use it from the fact that the ordinary amides of the body, of which urea is the principal one, are eliminated in an undecomposed state. When, however, the amide is in combination with a metal, decomposition occurs, and the metal is reduced and deposited. Liebreich found, by actual experiment, that this statement is true of mercury. It is supposed that the formamidated mercury, after hypodermic injection, undergoes decomposition, and that the metal mercury is set free in the tissues. The preparation is soluble in water, of neutral reaction, does not coagulate albumen, is not precipitated by caustic soda, and the presence of mercury can be demonstrated by potassium sulphide. It produces its effects very surely and rapidly. Liebreich regards it as the best remedy known for the hypodermic treatment of syphilis by mercury, as it is but little liable to excite local troubles or salivation. Later (Med. Times and Gazette, July 7, 1883), we find that Prof. Zeissel, in Vienna, after trial of this remedy in fifteen cases of syphilis, was well satisfied with the results. In three of these cases salivation was produced. Some pain followed its injection, which was not, however, so severe as that following the hypodermic use of mercuric bichloride. Twenty injections was the maximum number required to disperse the manifestations, even in severe cases.

Dr. Schacht, of Berlin (New Remedies, Sept., 1883), writes as follows:—

"Formamide is a colorless liquid, boiling at about 195° C., which can be distilled without decomposition only in vacuo. It is prepared by acting upon formate of ethyl by ammonia. When pure it is neutral, but easily becomes acid.

"If a *concentrated* solution of formamide be *boiled* with precipitated mercuric oxide, decomposition ensues, and metallic mercury is separated.

"On the other hand, if a *dilute* solution of formamide be *warmed* on the water-bath with precipitated mercuric oxide, a

clear, colorless solution results, in which soda (hydrate of sodium) produces *no* precipitate. Sulphide of ammonium, however, precipitates the mercury as sulphide, both from the formamide of mercury and from mercuric chloride. Solution of albumen precipitated by the latter salt, but *not* by the formamide.

"Formamide of mercury is prepared in the following manner: 10 to 13 grms. of freshly precipitated, completely washed, and still moist mercuric oxide are gently warmed with a little water in a porcelain capsule, with gradual addition of 10 grms. of formamide. As soon as solution has taken place, the resulting colorless liquid is filtered into a litre-flask, and the latter filled to the litre-mark with distilled water. Each cubic centimetre contains 0.01 grm. of mercury, which is the quantity representing *one* hypodermic dose. Formamide of mercury keeps well in brown-colored bottles, and should also be dispensed in these."

In a note from Vienna (Med. News, October 13, 1883), it is stated that Prof. Neumann is now trying hydrargyrum formamidatum on a large scale as an anti-syphilitic. It is used hypodermically in doses of 1 c. c. It acts with far greater efficacy upon the recent efflorescences than upon the later manifestations. Pain of great severity and active local inflammatory troubles have resulted in Neumann's cases.

The preparation which I exhibit, made by Merck, of Darmstadt, is a one per cent. aqueous solution, and the dose of it is from half to one ordinary hypodermic syringe-ful.

It has not been possible to obtain this drug in this city until the present time. I would be glad to place the specimen which is exhibited, in the hands of any member of the Society who may desire to make a trial of it clinically.

DISCUSSION ON HYDRARGYRUM FORMAMIDATUM.

Dr. Shoemaker: This paper is of much interest to me, as I have been using for some years, with good result, the hypodermic method of treating syphilis with corrosive sublimate. I have, however, found that the corrosive sublimate in from one-tenth to one-half grain doses, increased or diminished in dose according to the requirement in each case, was sufficiently effective in managing the majority of stubborn cases of syphilis. If the hypodermic syringe is in good order, as well as the needle, a gold one being preferable, and the operation is performed in a careful and skilful manner, no abscesses or ill-effects can or will follow the injection, I have treated many cases after this manner with the most happy effect, and cannot see that the remedy presented possesses any advantages over corrosive sublimate.

RELATION OF PHYSICIANS TO LIFE INSURANCE.

Read October 24, 1883.

BY JAMES FARRAR STONE, A. M., M. D.,

Lecturer on Physical Diagnosis in the Medico-Chirurgical College of Philadelphia.

IN the year 1859, the regular life insurance companies of the United States, reporting to the New York Insurance Department, had in force, in round numbers, 50,000* policies, representing a total insurance of \$140,000,000. At the close of 1882, according to the same authority, the number of policies in existence exceeded 660,000, insuring more than \$1,600,000,000. During last year, these companies, thirty in number, issued about 92,000 new policies, and distributed among their policy holders or their representatives over \$52,000,000—an average of almost \$150,000 for every day of the year, Sundays included. Nor do these figures, large as they are, fairly measure the growth and extent of this business.

Within the period covered by these statistics, and not included in them, new forms and methods, calculated to bring insurance to the notice and within the reach of all classes in the community, have been devised and introduced with such success, as to make it altogether safe to assert that but a moiety of the volume of business, and a yet smaller proportion of the persons carrying insurance of some sort, is represented in the returns quoted above.

Under what is known as the Prudential or Industrial system, it is possible for the laboring man, by the payment of a weekly pitance, to carry on every member of his family above two years of age, policies of insurance, differing only in amount from those issued by the regular life companies. Hundreds of thousands of such policies have been issued within a few years, and the number is rapidly increasing.

Co-operative societies, though by no means a novelty in insurance, under the taking, but fallacious, cry of "cheap insurance," and by a skilful and exaggerated parade of the defects and failures of the "old line" companies, have attained of late a widespread popularity and an astonishing growth. Accurate statistics as to their membership are lacking, but the true figures, could they be given, would doubtless exceed the largest estimates.

* Insurance Year Book, 1883, p. 446.

And what of the future of this business of life insurance, which has reached such gigantic proportions in a period so marvelously brief? Having come to this growth so speedily, has it already passed its maturity, and is it destined to as speedy a decline and collapse? Such is not the judgment of those best qualified to speak on this question.

Whatever may be the fate of unsound and speculative experiments—and that fate is neither uncertain nor remote—legitimate insurance is fraught with too many advantages, positive and self-evident, and has too often proved its power to help in time of need—when other help was not—ever to fall into disuse among a people who have once experienced its benefits. Indeed, speculation on this point is idle.

This institution, which had its birth in the eighteenth century, is still but a child in the nineteenth, while all signs point to a coming manhood, besides whose vast proportions the developments of to-day shall seem puerile indeed. The day may come (why not?) when the voice of the agent shall no longer be heard in the land, because there will remain none unconvinced of the duty and privilege of life insurance. In that day every prudent citizen will as naturally insure his life, as to-day he does his property, and not to be insured will be regarded as *prima facie* evidence of an unsound body, or an ill-balanced mind.

So potential a social factor is worthy the careful study of every thoughtful man. As conservators and promoters of the physical, and by necessity also of the moral, well-being of the community, physicians are especially called upon to decide what their relations to it are or should be. Because the relation of the medical profession to life insurance, as an institution, is seldom publicly discussed, and because I deem the connection both intimate and important, I crave your indulgence while I refer, as briefly as possible, to this portion of the subject.

My first remark, in this connection, is that life insurance was, in its origin, essentially an outgrowth of medical science. The idea of insurance may be traced among the ancient Phœnicians and Israelites, and is probably coeval with the establishment of settled communities, but the application of it to human life is a modern invention. Nor is the lateness of such application a matter of wonder. How chimerical must have seemed the notion of founding a business enterprise on that most uncertain of

chances—the length of an individual life. Whose brain it was that first conceived the thought, we do not know, but we do know that until the law of the absolute uniformity in the average duration of life, when large numbers are compared, was demonstrated, life insurance, as a science or a business, was an impossibility.

The mortuary records and observations which furnished the data from which this law, the corner-stone of life insurance, was at length deduced, were mainly the result of the labors of the medical profession.

Dr. Allen, in the introduction to his work on “Examinations in Life Insurance,”* well says: “It [life insurance] has its very basis and foundation in the established laws of mortality, as carefully and patiently worked out by medical men. The first life company was only started after Dr. Halley, of London, had made that series of observations regarding the duration of human life, out of which grew the ‘Breslau table of mortality.’ Every important step in life insurance has been preceded by a pioneer corps of physicians, who have carefully marked out the way, and, in no single instance, has future experience proved the falsity or unreliability of their conclusions.”

Not only is this true of the past, but life insurance is still dependent upon medical aid for its safe existence and healthy growth. If every person who passes the office door of a company could be insured, it might be safe to dispense with any medical test, for, in this way, the average health of the community would be represented in its risks. But if the doors were thus freely opened, who does not know that the old, the halt, the maimed and the blind would crowd its friendly portals, while too many of the young and vigorous would pass carelessly by on the other side. To protect itself against this adverse selection the company invokes the aid of the physician. It will need his aid until human nature changes, or every one insures.

The medical profession may well be proud of their share in originating and preserving so beneficent an institution. Whatever mistakes or frauds have impaired its usefulness or brought discredit on its name, none are chargeable to their fault or faithlessness. The wildest schemes, the most brazen-faced impostures, find it necessary, in order to obtain a hearing, to employ medical examiners, and to pretend conformity to the laws established by

* “Medical Examinations in Life Insurance,” p. 5. T. Adams Allen, M. D.

medical experience. The physician has need to be on his guard, lest he unwittingly lend his influence to unworthy schemes in accepting an appointment, as medical adviser, in a company of whose plan and character he is ignorant. Many reputable medical men, in this State, have been seriously compromised by their connection with the infamous "death-bed" insurance concerns, now happily in the final throes of dissolution. In consequence of unfortunate experiences, not a few have altogether lost faith in insurance, and include all organizations in their unqualified denunciations. In spite of the wrecks, marring the whole history of insurance, carrying loss and suffering into so many homes—in spite of the frail crafts, now venturing forth in such multitudes on the alluring, but treacherous, seas of voluntary assessments, with no adequate safeguards against the slowly swelling billows of mortality the lapsing years are sure to raise—whose voyage must end in shipwreck, if there be any reliance in the warnings of experience, any truth in mathematics or any permanence in human nature—there is yet, I maintain, good and sufficient ground for confidence in the safety and stability of legitimate life insurance. Moreover, I claim it to be the duty of every physician, as it certainly is within his power, not only to discriminate between the honest and the fraudulent enterprises, but also to determine which among the former are best worthy of his support. This he should do for his own sake, and quite as much for the sake of those who will be influenced by his example.

Nor is this so difficult a thing to accomplish as many suppose. The hue and cry, that has been raised over exceptional instances of corruption and mismanagement, has attracted too much attention to unfortunate details. The dust of dispute over minor points has blinded the eyes of many to the grand results accomplished. A study of its record ought to silence every cavalier and convince every honest doubter of the beneficence and reliability of true insurance.

But what are the marks by which legitimate insurance may be recognized?

Although by no means an expert in this part of the subject, I shall venture to mention a few of what seem to me to be useful points in the diagnosis. It will be understood, of course, that I am not speaking of any special company, but of the kind, or plan,

of insurance worthy of confidence. Similar tests, however, are available to decide upon the merits of an individual company.

The first test I would apply is that of *longevity*. No man cares to insure in a company likely to die before himself. No man ought to insure unless convinced of the stability of the organization assuming his risk. How shall this point be determined? Theoretically, no human enterprise has a surer basis. Death is the one certain event in life, and, out of many lives, the number that will die in any year can be foretold with an accuracy pertaining to scarcely another future event: 100,000 persons, ten years of age, will live a definite number of years in the aggregate; of the 100,000,* 85,000, in round numbers, will be living at the age of thirty; of these, 720 will die before reaching thirty-one; at forty, 78,000 will be alive, and 765 will die in that year; at fifty, 962 will die out of 70,000 survivors; at sixty, 1546 out of 58,000 who are left; at ninety, the mighty host has dwindled to a forlorn hope of 847 veterans, almost half of whom will pass away before their ninety-first birthday. It will be seen, from this table, not only that there is a definite mortality for each year, but also an increasing ratio of deaths with advancing age. Provision must be made for this, or there can be no guarantee of permanence.

The problem of a sufficient rate of premium is a purely mathematical one, embracing estimates for expenses, rates of interest, etc., as well as the mortality rate, and need not detain us further. The point to be insisted on is that *some adequate* provision shall be made to meet this inevitable experience. No company that fails to do this is worthy of the name it assumes.

But theory is one thing and hard facts quite another oftentimes. "By their fruits ye shall know them," is as good a test in insurance as in morals. What is the testimony of history as to the longevity of companies which have made proper provision for increasing mortality? Of the three life companies first formed in England, more than 160 years ago, two are alive and sound to-day. In this country the oldest companies are the largest and strongest, with hardly an exception. It will be urged this may all be true of surviving companies, but it does not take into account the multitudes founded on the same principles which have perished, leaving widespread losses and bitter disappointment. The fact of

* American Experience Mortality Table.

failure, the worse fact of fraud, in numerous instances must be admitted. No human institution can claim exemption from this experience. But a wide survey of the field deprives this fact of any weight as against the system.

In view of the gross exaggerations persistently dinned into the public ear from unworthy motives, this well authenticated statement may seem incredible: "Taking all companies, good, bad and indifferent, into account, it is claimed that *less than one per cent.* of all money ever invested in life insurance in the United States has been lost through mismanagement, dishonesty, failure or other cause."* What business can make an equally favorable exhibit?

Another test is the "expenses of management." The State Insurance Reports offer a ready and authoritative means of ascertaining exactly what these are in any regular company. According to the report of the Insurance Commissioner of Massachusetts for 1883, the ratio of expenses to "mean amount insured" in all the companies doing business in that State in 1882, was .79 per cent.—less than one per cent. The expenses of some of the largest and oldest companies in this country, from their organization to the present time, have averaged only about twelve per cent. of their receipts. The following summary, compiled from the sworn reports of twenty-three American companies from their beginning business to January 1, 1883, presents the facts in a more striking light:

The whole amount received from policy holders,	\$1,075,000,000
Paid back to policy holders,	782,000,000
On hand and invested for policy holders,	429,000,000
Total paid back or available for policy holders,	1,212,000,000

or \$136,000,000 in excess of the premiums received after deducting all expenses. Out of this excess, it is estimated, could be made good to every individual his actual money loss through all the failures of regular insurance companies in the United States, and that too without impairing the reserve of the surviving companies.

Still another test is the fairness and promptness with which death claims are paid. No one wishes to bequeath to his family a lawsuit as an appendage to his policy. Yet the fear of this very thing haunts the mind of many a man as he reads the com-

* "Life Insurance does Assure," page 22. S. H. Tyng, Jr. Coby & Co., N. Y.,

ments of the press upon some suit in the courts over a contested claim. How groundless is such a fear may be seen from the fact that not one policy in one hundred that become claims, are resisted or compromised in any way. In the interest of good morals, as well as in behalf of the rights of policy holders, some claims ought to be disputed. The danger lies now in the opposite extreme.

The only other test, to which I shall allude, is the contract or policy offered as the basis of insurance by the company. Insurance is a commodity, and like any other purchasable article may or may not be worth the price asked. The man who buys without examination has only himself to blame, if he finds himself possessed of what he does not want. It is astonishing how careless men often are in the matter of insurance, who in other transactions are notably shrewd and cautious. They will invest their money on the mere word of a glib talker, taking little or no pains to verify or disprove his assertions, and then even neglect to read the contract to see if it contains what was promised. No agreement which leaves in any doubt the amount and guarantee of insurance, or its cost, is deserving the name of a policy. Yet one or both these essentials are lacking in many so-called policies of insurance.

In the past grievous, and too often well-founded, complaints have been made of the restrictions and technicalities, on account of which policies have been forfeited, and the savings of years confiscated without redress. But, like many another evil, this has worked its own cure. No one hereafter needs to suffer loss on these grounds. It is possible to obtain a policy which will not only protect the owner against injustice in case of inability to fulfil his obligations, but which will contain within itself, in actual figures, the sum which he is entitled to receive in cash, or in paid-up insurance, in any year in which the policy shall lapse through non-payment of premiums for any cause whatsoever.

I need not dwell upon the arguments in favor of insurance as a promoter of habits of thrift, or dilate upon the beneficent results to the general weal, in providing for the support of so many dependent ones, who would otherwise become a burden to the community. Although these are excellent reasons why the attitude of physicians should be a friendly one toward the institution, they are too familiar to require repetition. I will only call atten-

tion to the fact that medical men profit directly and largely in the payment, from *the proceeds of policies*, of professional services, which would otherwise be unrequited.

Important contributions to vital statistics may be confidently anticipated from the rapidly accumulating mortuary experience of the companies. These records are kept with a care and accuracy unattainable elsewhere, and the deductions drawn therefrom will be correspondingly valuable.

Already it has been demonstrated that the average longevity in civilized countries is gradually rising. The experience of American companies* seems to be slightly more favorable than the British, but it would be premature, as yet, to conclude that this is due to greater longevity in the community at large. Indications point to a slight decrease in the ratio of deaths from phthisis, and an increase in those due to kidney affections. That greater results have not already been reached is owing to the brief time and limited experience covered by the records. These observations must throw light upon such questions as the greatest duration of human life, the geographical distribution of diseases, the effects of race, occupation, hereditary tendencies, and physical condition and configuration, and many kindred topics.

With respect to the special relations the physician assumes in becoming the medical examiner of a company, and his duties in that capacity, time is left only for a most general and cursory glance. It is at once the most practical and difficult part of my subject. To treat it adequately would require hours instead of minutes.

Regarding the ethical relations of the position, it is evident that he owes paramount allegiance to the interests of the company whose officer he is. The well-being of the company—and by this term is not meant any corporation, but the whole membership constituting it—demands the maintenance of a longevity at least equal to that of the general population. Under present conditions, those seeking insurance fall below that standard. This adverse selection the medical officer is appointed to prevent. To fulfil this trust he must weigh every candidate in the scales of physical fitness alone, and be blind to every alien consideration whatsoever. But while thus loyal to the company, he must not lightly regard the claims of the applicant. To deny to any man

* Rep. Mortuary Experience of the Mutual Life Ins. Co. of N. Y., 1843-1874, pp. 8, *et seq.*

the privilege of insurance is never a trifling thing, and it may be a serious and lasting injury. It becomes an outrage if based on any improper motive.

It behooves the examiner for *his own* sake to make no mistake. His verdict is sure to be reviewed, if not by the examiner of a rival company, at least by the candidate's private medical adviser. In the latter case a reversal of his judgment may generally be anticipated, no matter how manifest the disqualification. He should cherish no resentment against his brother practitioner on this score, however, remembering how prone we all are to prophesy smooth things to our patients.

How shall we decide, in view of the conflicting claims of the applicant and company, those border-line cases, which so often arise? But one reply can be given. When doubt remains after full investigation, always give the company the benefit of the doubt. When compelled to deny an application for insurance, the examiner may occasionally render a more than compensating service to the candidate, by revealing his timely discovery of an unsuspected disease, amenable to treatment in its early stage. Many valuable lives have thus been saved, or prolonged, and it is worthy of mention as an incidental benefit of insurance examinations.

The relation of the examiner to the agent ought to be, and generally is, one of co-operation, yet of absolute independence. The aim, the true interests of both are identical, however they may sometimes seem to clash. But the motives that sway the one must never be allowed to influence the judgment of the other. Their mutual action—to illustrate a small matter by a great—may be likened to the centripetal and centrifugal forces, whose resultant motion is the smooth and noiseless sweep of the planets in their orbits. So the agent and the physician, working harmoniously from different directions, give impetus and safety to the chariot of insurance.

The last topic on which I shall touch—and I cannot do more on this occasion—is the important one: How can the medical examiner best discharge the practical duties of his office? The first and obvious answer is, by having a clear conception of what those duties are. In general they may be comprised in the obligation to recommend none but healthy lives for insurance. But inasmuch as perfect health is a condition most rarely, if ever, met

with in actual experience, it is evident that something less than this ideal standard must serve for his working rule. What shall it be? Some companies have solved this problem by establishing a sliding scale, according to which all risks are graded into classes, extra good, good, fair or average, and poor. Others encourage the examiner to report mainly on the physical condition and habits, leaving points of hereditary influences, and other general questions, to be decided at the home office. This plan relieves the local examiner of a certain amount of responsibility, and has the apparent advantage of referring to experts the weighing of points, upon which the average medical man has had but little experience. As the whole application comes under the review of the medical board in any event, it may be questioned whether the judgment formed from personal contact, in view of *all* the circumstances affecting the risk, is not of more value to the company than one with certain elements ignored.

I cannot do better, just here, than to quote the formal question, closing the medical examiner's certificate on the application of a prominent company: "Is the person, in your opinion, as good a life for insurance as the average of persons of *the same age, who are of sound constitution, in good health, and whose family history is good*, and do you, acting in the interest of the company, advise the acceptance of the risk?" *

To my mind this is the most fair, logical and comprehensive statement of the object of the examiner's work with which I am acquainted. To answer the question fairly requires a balancing of all circumstances affecting the life favorably and unfavorably, and an unqualified decision upon the relation of the risk to the standard assumed. With a clear conception of the purpose of his examination, the physician will do well to remember that the applicant for insurance stands in a relation, the very reverse of that occupied by a patient. The latter comes for relief, and is ready to aid, so far as he can, in the discovery of his ailment. The former approaches with the assumption of health, and the examiner must detect, unaided, any fallacies in that assumption. The task is, not infrequently, made vastly more difficult by the deliberate purpose to gain the end by deceit.

The application blanks of different companies, while covering

*Application of the New England Mutual Life Ins. Co., Med. Examr. Certificate, Quest. 9.

virtually the same ground, vary greatly in details and in the prominence given to special points. It would seem to be entirely practicable, and would certainly be a gain in obvious respects, if all companies would agree upon an identical form. Formerly the answers to questions, covering the family and personal history, were filled out by the agent. The tendency is now to include these in the examiner's certificate, as is most proper. The application is the basis on which the insurance is granted. It contains, in the vast majority of cases, the only information accessible to the company, on which to judge the character of the risk. In filling it out the examiner, with this fact in mind, will endeavor to make it a truthful and complete description of the applicant and his environments, as he sees him. A golden rule to be observed is brevity in noting normal conditions, fulness and clearness in describing abnormal states.

The order usually outlined on the blanks is both the natural and philosophical one. It is, first, the personal history; second, the family history; lastly, the personal examination. Much depends upon the tact and perseverance of the examiner in eliciting information of value under the first and second heads. Successful cross-examination is an art to be mastered only by much practice.

One of the knottiest questions to answer satisfactorily, in many instances, is that respecting the applicant's use of liquor and narcotics. The importance of the information sought is commensurate with the difficulty of obtaining it. According to Neisson's statistics, as quoted in "Parkes' Hygiene,"* "In intemperate persons the mortality at twenty-one to thirty years of age is five times that of the temperate; from thirty to forty it is four times as great, becoming gradually less with advancing age."

As total abstainers are, unfortunately, as rare among insurance candidates as in society at large, the question as to a temperate or intemperate use of liquor comes up for decision in almost every case. But what is a temperate use? "To some"—I quote from an English writer on insurance—"a question of the quantities in which, and frequency with which, a stimulant is consumed. To others, of ability to take large quantities without apparently losing control of their faculties. To the expert—who knows that to a very large proportion of human beings the smallest use, except as

* Parkes' Practical Hygiene, fifth edition, p. 291.

medicine, is injurious, and that what to a large class of men appears to be moderate indulgence is too great to remain without effect in seriously perverting nutrition—a very different question. Whatever may be our doubts as to the injurious effects of small quantities of narcotics and stimulants upon the system, we should have little hesitation in declaring that when they are long used, and in large quantities, they must, in the vast majority of cases, impair health and shorten life, so that a life so exposed needs higher rates.”

Practically, then, it is not sufficient to trust to general statements, but it is important to learn just how often and in what quantities stimulants are taken. With all possible care, the examiner will sometimes be most egregiously deceived.

The family history presents difficulties, often insuperable, in the lack of knowledge of the causes of death among the immediate relatives. When we push our inquiries a generation further back, it is remarkable how little the average citizen can tell of his grandparents. Yet much may be learned or excluded by persevering inquiry. It is worth the effort if it be true, as Ribot asserts, that “Longevity depends far less on race, climate, profession, mode of life, or food, than on hereditary transmission, * * and will assert itself above many influences generally fatal to a high average duration of life.”

It is in the physical examination that the best work of the examiner is demanded. Closeness of observation, system, thoroughness, accuracy, must be his watchwords. He must insist on favorable surroundings, quiet and privacy, on time sufficient for examination without haste, on repeated interviews when needed, and, above all, on entire freedom from outside pressure or dictation in making up his verdict. He owes it to himself, and to the trust confided in him, to keep fully abreast of the advances in medical knowledge, that he may avail himself of every means likely to throw light upon the difficult task required of him. He should be familiar with the various “instruments of precision,” that he may employ such as may be of service in detecting obscure morbid conditions. But his whole duty is not fulfilled in the recognition of existing morbid states. It is required of him to detect latent tendencies to disease, to foresee the coming evil in the shadow it casts before, to apprehend the signs which betoken the threatened tempest. What wonder, then, if he often fails in

the accomplishment of such a task? No man, I verily believe, ever long occupied this position without acutely realizing the limitations of human knowledge, the inadequacy of his ability to read aright the premonitions of decay—nay, even the certainty of his failure, now and again, to discover existing evidences of disease. His experience would be different from mine who never meets on the streets men, vigorous and strong, whom years ago he rejected as doomed to premature decline; or who cannot recall others whom he accepted without a misgiving, who, too soon for his self-complacency, succumbed to influences which he failed to detect.

It is no flowery bed of ease, this post of insurance examiner, where one may betake himself to peaceful slumber after receiving his fee. Ghosts of his old mistakes are likely to visit his couch at most unseasonable hours, and at any moment he may be rudely disturbed by the untimely demise of some recent risk he had rated as extra good.

There is left for the conscientious examiner, at least the approval of his own conscience for work faithfully attempted, and the assurance that his labors, with those of his compeers, have thus far successfully accomplished the purpose intended.

What, then, have been the results of the medical examination of lives, as tested in the experience of American companies? In general the medical selection may be said to effect a diminution in the death-rate among insured lives as compared with the whole population, which continues at a lessening ratio for about six years, after which its influence disappears, and the mortality approximates that of the community at large.

Three propositions are regarded as established in the experience of the largest American company:*

1. That the advantage of selection diminishes at all ages with the duration of the policy.
2. That it decreases very rapidly among those who insure at the younger periods of life.
3. That it decreases more slowly at middle life and among older insurants, and probably never entirely disappears.

1922 MT. VERNON STREET.

* Report of the Mortuary Experience of the "Mutual Life" of N. Y., from 1843 to 1874, pp. 18, *et seq.*

DISCUSSION ON THE RELATION OF PHYSICIANS TO LIFE INSURANCE.

Dr. E. Stanley Perkins, in opening the discussion by request of the Chair, said: It is safe to say that there have been organized in this country from 250 to 260 old-line companies, of which there are now about fifty-two in existence; a part of which would be far better, in view of the condition they are in and the benefit they are extending to their policy holders, in some other condition than that of mere existence. Almost the entire old-line business is now being done by twenty-five or twenty-six companies. Of these 220 companies which have ceased to do business or fallen into a *moribund* state, it is, of course, impossible to say that *all have failed*. A great many of them have evaded failure by transferring their risks. In the majority of cases where these transfers have been made, they have been of such a character that it would have been far better for the policy holder if the company had failed. As to the causes of failure—dishonesty, the organization of offices for which there is no need, high salaries to officers, wasteful expenditure of moneys, may be mentioned. The president of one of the largest *old-liners* in New York testified under oath before the Legislative Investigating Committee that he was paid the wretched “bagatelle” of \$7500 a year as salary. To be sure, he admitted that he had a little allowance—a sort of reward of merit—of \$50,000 more, and then, in order to encourage him, he was allowed to receive \$20,000 in addition from another source. But he only got \$77,500 altogether, and the thirty-nine other officers of this corporation starved on salaries which ranged all the way from the sum for which the poor president slaved for two or three hours a day, down to \$1995 per annum. But aside from all this, the root of the matter is that the system itself is wrong. It presents a premium made up of three parts, one for *expenses*, one for *current death-losses*, and one to be put away as a reserve to reduce the amount which is at risk, or to meet the future, as they call it. They know as well as they are born that the average life of a policy is seven years, yet they will, right from the commencement, charge you a *reserve* which is enormously too great, and which is based on your living in the company thirty or forty years.

Now what is the actual fact in regard to this money? It is massed together under the name “annual premium.” There is not a word said as to one portion being for expenses; not a word as to one portion being for current death-losses; not a word as to one portion being for a reserve or trust fund. It is all jumbled together and paid as a premium. There is nothing in the nature of a trust fund from the beginning to the end. All the money goes into the hands of men who are entirely irresponsible, so far as retaining any of it as a special trust is concerned. In this connection it is interesting to read the testimony of the president of one of the “old-liners,” given before the Legislative Committee in 1877 (see page 67, official stenographer’s notes):—

Q. How much have you had in the bank for the past six months? A. We have variable amounts, from half a million to a million and a half.

Q. When money is paid out by the company, what is the method of paying it out? A. It always goes to the finance committee first, and secondly to the two officers.

Q. Suppose you presented a check to the bank, by whom would it be signed; or, in other words, if your association was drawing a draft on the bank for \$10,000, by whom would it be signed? A. By two officers.

Q. What officers? A. Any two officers; by officers I mean the presidents, vice-presidents, secretary or actuary, or chairman of the finance committee.

Q. Two of either of those could draw any amounts? A. Yes, sir; they are competent to draw money out of the bank.

Q. Any amount you chose to draw? A. Yes, sir.

Q. You say it requires the action of the finance committee; is there anything to indicate to the bank upon which you draw that that action has been taken, except the signature of the two officers? A. No, sir.

Q. Nothing whatever? A. No, sir.

Q. So that if to-morrow your assets are a million and a half, and a check should be presented, signed by yourself and either of the other officers of the association, that amount will be paid upon the check. A. Yes, sir.

Q. Is there any direction to the bank, or any instructions, which forbid their paying an unlimited amount upon the check of the two officers of the company? A. No, sir.

In the very nature of the business, what is the first thing that is done with the money which thus finds its way into their hands? Expenses are paid out of it. That is the first thing. There are no death-losses paid, not a cent put away as reserve, until the *expenses* are paid. The next thing is the current losses. These must be met in order to keep the business going. Then the balance, such as it is, can be put away as a reserve—excepting that the exigencies of the business require that there should be some pretense made of paying *dividends*, regulated by a competitor over the way. You must pay a *dividend* that will correspond with what he pays, or at least make the public think you do. This *reserve*, which amounts to such a large sum in the course of years, and which had better be kept in the pockets of the policy holders for their own use in business or otherwise, is put into the hands of an irresponsible management, without anything requiring it to be regarded as a trust fund. What other result could you expect than wastefulness, dishonesty, and mismanagement. These moneys *are* trust funds, and the State should see to it that they are made such. We may talk about the companies being *mutual*, but every one knows what mutuality on the old-line plan means. It means a half dozen men, with their pockets full of proxies, electing themselves each year to the positions of directors or trustees; making reports to themselves as to the conduct and condition of the business, approving their own reports, and then going on again with the business. Will these men who are to-day controlling \$450,000,000 of trust funds come forward and say “We are not responsible,” “We are not able to take care of this money,” “For your

own sakes, take it out of our hands." It is not in human nature. It must be done by the 750,000 policy holders of these companies.

Now let us turn from our own country. What has been the history of old-line high-rate companies in Great Britain? In 1844, Parliament passed the "Joint Stock Companies Act," under which all companies organized were required to report. From 1844 to 1866, 258 companies organized, of which number 214 had dropped out of existence in 1866, of which the following record is given :—

Failed to complete organization,	6
Died for lack of business,	5
Dissolved,	8
Swindlers,	10
Winding up in chancery,	17
Transferred,	96
Transferred and re-transferred,	72
Still in existence,	44
	<hr/> 258

The old-line companies reporting to the Insurance Department of New York in 1882, issued 91,945 policies and lapsed 36,207 ! Over one-third of a year's business lapsed at the end of a year ! Is it any wonder that the *average* life of a policy is not more than seven years. I know a company in this city (and so does Dr. Bruen, for he examines for it) where, last year, the new general agent strove to make a record by getting in new business. In many cases almost all the premium was thrown off in favor of the applicant, and I don't think any of them paid more than one-half the year's premium for a whole year's insurance. I, myself, received a policy of \$1000 for a year's insurance (Dr. Bruen examined me) and *it didn't cost me a cent*. Where did Dr. Bruen get his fee for examination? I didn't pay it. Who did? Neither Dr. Stone nor Dr. Bruen need make any apologies for examining for such concerns. We don't inquire into the moral character of our patients or those who employ us. We simply do our professional duty and pocket our fees.

The experience of the large life insurance companies for the last thirty-five years, shows that it costs only \$9.55 per year for that time to carry \$1000 insurance on a man aged forty years. Yet they persist in continuing to charge \$39.52 for that amount per year. The exhibit of twenty-seven companies last year is as follows :

Gross income,	\$60,856,936
Amount paid policy holders,	36,911,310

In other words, fifty-two per cent. was paid for death-losses and forty-eight per cent. for expenses and accumulations.

A far worse showing is presented in the Pennsylvania insurance reports for the past ten years, which show that during that time the old-line com-

panies have collected \$699,250,701, and that they paid in losses \$285,354,004; the difference of \$413,896,797 has gone to the expense account or been held in the hands of men who assume no responsibility for it. A few days ago I read in an old-line life insurance journal that there is no difference between a mutual life insurance company and a co-operative company except *that of method*. But that is the same principle which in other stages of the world's history has given rise to religious persecutions that have deluged the world in blood. *Then* it was only a difference of method! But the men who held power were determined that those who would not seek heaven by their *method* should be deprived of the power of seeking it by any method; and to-day the old-line monopolists are resolved—their wrongful accumulation of money helping them—that men who do not take life insurance according to their method shall be prevented from receiving its benefits in any way.

The oldest old-line company in America has existed but forty years. Out of 252 such companies only thirty remain, and three—the Mutual Life, the New York Life, and the Equitable Life—secure more than one-half of the entire new business in this country.

Before leaving the old-liners, let me call your attention to a case which will show you how much of the assets of an old-line company belongs to the policy holders: In the case of *Bewley vs. The Equitable Life Assurance Company*, New York Supreme Court, July term, 1881, the plaintiff, who was a policy holder, sued the company for his proportional share of the surplus or assets of the company, setting forth in his petition that these profits were divided among officers, and that the members were not receiving their share of the earnings, profits, or surplus. To this claim the president of the company demurred, and set up as a claim against the plaintiff the following: "The plaintiffs, as policy holders, have no rights which entitle them to bring this action; the policy holder is not a partner; he is not a creditor; he is not a *member* of the company. The fund produced by the payment of all the premiums does not in any sense belong to the policy holder, but belongs exclusively to the company." On argument, the demurrer was sustained and the petition dismissed. What safety is there, then, in having a company accumulate \$69,000,000 of assets?

Co-operative Societies.—It is said that co-operative assessment societies must die; that mathematics or actuaries' tables will not warrant us in believing that they can exist, by reason of the advancing age of their membership. I say that the old-line companies were founded upon the experience of these co-operative societies. Let us go to England for our experience.

How long have these English societies existed? The Chief Registrar of the friendly societies, in his report to Parliament, ordered by the House of Commons, August 2, 1881, answers this question. Below will be found the names and date of organization of a few of these friendly societies, as per the Registrar's report:

Name.	Date of Organization.
Count De Winton Society,	1168
Loyal Evanus Society,	1358
Norman Society,	1703
The Society of Lintot,	1708
The Amicable and Charitable Society,	1759
The Armley Clothiers' Loyal and Friendly Society,	1760
The Meriden Friendly Society,	1772
Old Spread Eagle Friendly Society,	1762
The Amicable Society,	1779
Sandon Junior Friendly Society,	1772
Norton Friendly Society,	1760
Kingsly Friendly Society,	1768
Whittington Men's Friendly Society,	1754
Wheat Shief Friendly Society,	1765
Earl Shilton Friendly Society,	1714
Royal Artillery Society for Widows and Orphans,	1752
Canterbury Friendly Society,	1737
Beer Male Friendly Society,	1763

And there are sixty other friendly societies, all organized and established before the present century.

There are 10,755 friendly societies in England, with a total membership of over 7,000,000 ! The Royal Liverpool Friendly Society has 865,000 members ; the Liverpool Victoria Friendly Society has 472,000 members. The oldest friendly society has existed seven hundred and fifteen years ; the next oldest has existed five hundred and twenty-five years ! Millions upon millions of dollars and hundreds of thousands of claims are paid annually by these friendly societies.

They do not transact business under the level-premium system.

From the report of the Chief Registrar of Great Britain, published in 1881, we find that there are 10,755 societies reporting to his office, with a total membership of all societies of 7,000,000. The Chief Registrar shows that the mortality experience of the Manchester Unity, which covers 271 districts, with 1212 branches, some of said branches being established as early as 1814, for the period of five years from 1866 to 1870, inclusive, with 1,321,048 lives exposed, was but 1268, or about $12\frac{1}{4}$ deaths per annum for each 1000 members, which represents the mortality over fifty years after the first branch was established.

By the same report it will be found that the mortality of the Ancient Order of Foresters, with 139 districts, 1401 branches, first branch established 1831, from 1870 to 1875, forty years from date of establishment of the first branch, with over one million lives exposed, was 1214 per 100,000, or say 12 deaths per 1000.

Names of a few prominent English societies, as taken from the Chief Registrar's report, 1881 :

NAME.	Date when Established	No. of Members	Assets.
Royal Liverpool Friendly Society, . .	1850	865,076	1,145,000
Liverpool Victoria Friendly Society, . .	1843	472,945	250,000
Ancient Order of Foresters,	1831	201,633	5,545,000
Manchester Unity Friendly Society, . .	1814	188,519	9,695,000
United Assurance Society,	1849	181,093	65,000
Blackburn Philanthropic Burial Society, .	1839	120,402	90,000

Total number English friendly societies, . .	10,755
Total number of members,	7,000,000
Total number French societies registered, . .	6,777
Total membership in registered French societies, .	1,065,507

By the report of the Chief Registrar of the English societies, we find that the oldest friendly society now existing and reporting to his department was established in 1168, under the reign of King John, a century before the English secured their bill of rights or Magna Charta. It is known as the Count De Winton Society, and has existed over seven hundred years. The second oldest is the Loyal Evanus Society, which was established in 1358—over five hundred years since. Eighty nine friendly societies by the same report are shown to exist that were established in the seventeenth century, many of them having existed for over one hundred and fifty years, while over 1000 of these friendly societies are over fifty years old.

In our own country, with the aid of the reports made to the insurance departments of a few States, we are enabled to present the following report, which fairly represents the growth, magnitude and beneficence of the assessment plan of life insurance :

Number of societies reporting,	293
Number of members end of year,	907,249
Number of lapses for the year,	99,853
Amount of insurance in force,	\$1,890,844,872
Number of deaths for the year,	5,927
Amount of death-losses during the year, .	\$12,581,096
Total amount of death-losses since organization, .	63,608,063
Expenses for the year,	1,761,564
Receipts for the same year,	16,144,644
Assets at the end of the year,	3,492,384

Time was when very few distrusted life insurance or the men who managed it. It was quite universally regarded as a system of public benevolence. *Public confidence should have been retained.* It has been lost only by the grossest mismanagement, coupled, too, in many cases, with the basest betrayal of the most sacred trusts. *Mortality rates never*

make a company fail, and *seldom* are they an important factor in accomplishing its downfall where risks are properly taken. Along the way, strewn with ghastly wrecks of so many old-line companies, should be set frequent sign-boards with fingers pointing to these unseemly ruins, and on these sign-boards should be printed in conspicuous capitals: "Results of extravagance, incapacity and fraudulent management." The system is not *wholly* bad; needlessly extravagant it indeed is; but with competent, honest managers, *some* returns are made to those who intrust their savings to it.

It is surprising to notice the favorable change in public sentiment towards co-operative assessment insurance, during the last few years. It is no longer regarded simply as something that may do to afford temporary security to that class of the community that can afford nothing better; but our business men, our professional men, bankers, brokers, lawyers, editors, all classes, not excepting agents and officers of old-line companies; are among the patrons of our assessment companies. The arguments, the slanders, the falsehoods, the quotations from the reports of insurance commissioners put forth from seven to fifteen years ago, circulated to injure its cause, have little weight with thinking men to-day. When these companies can furnish insurance at less than one-fourth the cost of old-line insurance on their cheapest plan; when they show an increasing membership from among the most intelligent classes of the community; when they show increasing financial strength from year to year, it will take more than argument to overthrow them. Incompetent management will bring some companies to grief, and the best conducted companies will for a time suffer in consequence.

Let us be determined that we will have from the several legislatures of our States, uniform, legal recognition; let us have every safeguard thrown around life insurance business that wise legislation can provide; let us by every means in our power promote the enlightenment of the public in life insurance matters; let there be a careful selection of risks, and an application of the best principles and methods; and, above all, let there be an application of sound business principles by capable, honest men, and assessment insurance has but looked upon the first rays of the morning light of its long and prosperous day.

Dr. B. Lee: I wish to refer to two points which have occurred to me during the discussion. I believe there is a life insurance organization almost entirely sustained by the Society of Friends. Could a comparison be obtained between the vital statistics of the organization and that of other companies not so limited, it might afford interesting evidence as to the influence of habits of temperance and mental serenity continued through generations upon longevity. The second point is, that a certain exclusive set of medical practitioners have laid claim to a lower death-rate existing among those who accept their method of practice, and on this ground companies have been started which claim that they can afford to insure those following this exclusive method at lower rates than could be given to other persons.

The wide range which this discussion has taken enables me to introduce a subject which I have very much at heart. I refer to the co-operative form of life insurance existing within the organization of this Society. It may not be known to many of the new members that in the "Mutual Aid Association of the Philadelphia County Medical Society," they have a means of rendering it certain that in the event of leaving their loved ones—their widows and orphans—unprovided for at the time of their own death, they will not be abandoned to the cold charities of the world, and this by an annual outlay altogether disproportionate to the assistance to be rendered. This institution, it is true, is on a charitable and not on a business basis. It does not at all engage that a bonus shall be paid to the family or heirs of every deceased member, or even that what he has paid into its treasury shall be refunded to them, but simply that in the event of their being in want they shall be cared for. Every one who joins it does so in the earnest hope and firm belief that those who are dear to him will never be its beneficiaries. He has therefore the comfortable feeling that his trifling contribution will go to aid in the support of the survivors of some brother in medicine less fortunate than himself. Those of us, however, who have been for a long number of years connected with this society know how illusory a physician's bright anticipations of pecuniary success often are. I say with the utmost confidence, that there is not a man present, I care not how lucrative his practice, I care not how safe in his opinion his investments, I care not how great his wealth—I say there is not one present who can afford to neglect the advantages which this association offers. I have said that it is on a charitable basis, and I wish particularly to remind those of its members who are here, and those who may be desirous of becoming members, that it must therefore depend for the means to carry on its work of mercy chiefly upon the gifts and bequests of the benevolent. No such organization among medical men in London, New York, or any other great city has ever been self-supporting. It behooves us, therefore, if we would soon see our finances in such shape that we may be able to meet the applications that are made to our directors, to lose no opportunity of interesting our wealthy and benevolent patients, or others over whom Providence may have given us an influence, in this most needful and laudable charity, that they may remember it in their bequests, if they do not feel disposed to contribute immediately to its funds.

Dr. Holt : The fine rhetoric this evening has cast a little tinsel over this subject, and given it an importance beyond what it deserves. We are told that life insurance is a gigantic institution ; but so are the Alaska Fur Company and the Standard Oil Company, yet they are inappropriate subjects for us to consider here. I admit that cases may arise under which a man may with advantage insure ; for instance, when he has a large family and nothing to leave to them ; but if he has the ability from his own means to provide for his family, he need not insure. At best, the companies give us but three per cent. returns for our money, and this is a very low rate. To be sure, insurance *management* is a profitable business. I do not

consider the functions of a medical examiner as a legitimate line of practice. A doctor's duty is to treat the sick.

The life-insurance system puts a premium on crime, on deception, fraud, and even murder. It has been said here that very few cases have been protested by insurance companies. This is simply because it would hurt the companies' standing to be too frequently in litigation.

Dr. Walk : I would like to ask Dr. Stone if, in his judgment, the fees for medical examinations are adequate, either in the old-line companies, or in the co-operative associations. It is my impression that in both cases they are not what they should be, considering the skill and time required.

Dr. J. M. Keating : The subject of the paper by Dr. Stone is one of great importance to medical men. It has been one of my duties to spend much time in reviewing the post-mortem papers of a large number of insured persons, and one cannot do so long before becoming thoroughly convinced of the great uncertainty of life, and the importance of using this means of investment. The study of the detection of the earliest symptoms or indications of disease, as also their prevention, is the true claim of medicine as a science, and we are daily impressed with the want of thoroughness in our examinations and the poverty of the means at our command, by witnessing the rapid development of disease in cases we had so recently pronounced healthy, in which its indications were only evident by reflection. But as medical science has given the statistics for averaging the longevity of healthy individuals, it has seemed to me that what is now wanted is an estimate of the longevity of certain chronic diseases. The text-books fail to supply this. Many applicants are refused by companies on account of diseases which are very slow in terminating life—probably one case out of, say, nine applicants cannot be received on the life-plan ; yet such individuals could be placed in a class with hundreds of others of their kind, paying premiums, which they would willingly do, based upon the average longevity of the disease from which they suffer. They are a class needing insurance. Heart-disease is a frequent cause for rejection, and yet some forms of heart trouble are long-continued. Every person living has a certain expectation of life, and yet, so far, on a large scale, the average longevity of chronic disease has not been determined on a sufficient scale to warrant a fixture of life-insurance rates.

Dr. Neff : The persons who present themselves before the life insurance examiner are ostensibly in good health, and if the examiner kept a close record of each applicant, we might gain some information as to the earliest signs of disease ; *i. e.*, before they present themselves to the practitioner. We see persons with signs of a tendency to disease often due to heredity, such as under-weight, high pulse, slightly accelerated breathing. The question as to predisposition to phthisis arises in these cases. It is also a question at times whether we are just in refusing insurance to applicants who are apparently in good health, such as cases of functional or exocardiac murmurs, which are at times present for an indefinite period in what may prove to be exceptionally good risks. A heart murmur, no matter what

its characteristics, is always a ground for rejection. This is generally laid down as a law. Yet I know of cases in which patients with aortic murmurs had no serious symptoms for twelve years.

Dr. Stone, in closing the discussion, said: As to the experience of the insurance company conducted by the Society of Friends, about which Dr. Lee asked, I must answer that I have no information. As to the homœopathic company, I am doubtful, but I think it has not been a success. In answer to Dr. Walk's question, I would say that in life-insurance matters, as elsewhere, fees are not what they should be, but there appears to be no difficulty on the part of the companies in getting medical examiners at the rates which they now allow.

ON THE USE OF CACTUS GRANDIFLORUS IN CARDIAC AFFECTIONS.

Read October 24, 1883.

BY M. O'HARA, M. D.

I WAS called to see Ed. O'Hara, æt. seventy-four, May 19, 1883; he had bronchitis and some œdema of the lungs; his feet were slightly anasarcaous; there was no kidney difficulty, though he passed but little water; he had a mitral regurgitant murmur; some irregularity of heart's action; occasional intermission; pulse 90; he had arcus senilis and atheromatous arteries, as shown in the radials and temporals. The diagnosis was dilatation and failing heart, compensation gone by. He was given digitalis, iron and nux vomica. He became more swollen generally, had orthopnœa, suffered very much, heart becoming very intermittent on the least effort. The treatment was kept up, with addition of saline laxatives for extreme costiveness. He was going downwards daily, and on June 22 the pulse was very intermittent, and only thirty-four beats to the minute; very water-logged in lungs and over whole body. I had only seen him at intervals of several days, but still kept up the digitalis, as it is accounted a sure means of restoring compensation to a heart failing from dilatation, after hypertrophy has gone to its maximum. I thought I could be no worse off with any other medicine, or make less speed, and I recalled the fact that I had seen in "Flint's Clinical Medicine," page 223, the statement made that the cactus grandiflorus, in from three-to five-minim doses, is a valuable heart tonic, and concluded to give it a trial. I ordered it in five-minim doses of the fluid

extract (Parke Davis & Co.), every four hours. In a few days every symptom improved, the dropsy disappeared, he could lie down at night to sleep. He has been taking the medicine now for five months—the last month fifteen minims, three times daily; he feels quite improved; the dropsy has left him; he has the mitral murmur yet, and some irregularity, but rarely an intermittent pulse.

I am satisfied if I had kept on with digitalis he would have died. Dr. McElroy saw him the day I was changing for the cactus, and considered him as a man dying from heart failure, and that he could not live for four hours. He expressed his amazement at seeing him alive and so much improved two weeks later. I also gave the history of the case to Dr. Eskridge, and asked him to visit the patient a month ago, but unfortunately have mislaid his note. On another occasion (a patient similarly affected) I used digitalis, and it failed me. Rev. Mr. V. has hypertrophy with dilatation, commencing mitral degeneration, also commencing aortic valve disease; has pronounced mitral valve regurgitation; he had violent palpitations, irregularity of pulse, and intermissions; pulse between 40 and 50. Here, I am satisfied, digitalis and convallaria aggravated matters, while the cactus relieved the pain, stimulated the heart, and removed irregularity. The heart never comes up above 50 to the minute, but the horrible feelings of death, with the sudden stoppages, are relieved. In this case there was considerable gastric disturbance, and I assisted with pepsin and strychnia, and I wondered why digitalis failed, attributing it in part to its irritating effect on the stomach, thus disturbing reflexly the heart.

In case of Mrs. Lynch—dilatation with failing heart from age, sixty-five years—I have used nothing else and it has satisfied me. This person had vertigo, anemia of the brain, dropsy, etc., all due to the failing heart, and the use of cactus inclines me to think it was a good cardiac tonic.

I recall one case of a fatty and dilated heart in which at one period before an attack of angina pectoris, digitalis had no good effect, yet after that it served very well in the case as a tonic for the heart. Digitalis is a cardiac tonic, acting on the nervous ganglia of the heart, influencing its muscular substance. We will only find out all its ways by clinical experience. I have tried caffeine for similar heart cases and sometimes received no benefits from it. Belladonna and cannabis indica have assisted me in these weakening hearts,

especially if I associated these remedies with strychnia. I introduce cactus to the notice of the Society, because I have found it to have been little used. Many physicians, if they have like experience to mine, must recall the fact that digitalis at times disappoints them, and I would ask them to try this under those circumstances as a substitute. Of course I have not had much experience with it, and I would like the result of my experience to be confirmed by that of others.

In nervous affections of the heart I find it very useful; palpitations and neuralgic feelings of soreness about the heart. There are two preparations, *cactus grandiflorus* (night-blooming cereus), and *cereus Bonplandii*, of apparently same qualities, the latter of which I have made no use. The only information I have obtained about these remedies is on a fly-sheet from Parke Davis & Co., Detroit, Michigan, which refers to its use; physicians under their own names, speaking of it eulogistically as a complete substitute for digitalis, merely from their clinical experience. This I would not be willing to concede, though we may meet cases which from idiosyncrasy or other unexplained cause, cannot be benefited by digitalis.

Dr. Fothergill says, p. 287, "The Heart and its Diseases": "The systole is more complete, the chamber is more efficiently emptied, and consequently it is not so soon refilled, so that the requirements of the ventricle in diastole correspond to the slower rhythmic discharges, and a slower pulse-rate is established, while the pulse is firmer and less compressible, the arteries are better filled with blood; at times digitalis will notably lower the pulse-rate under other circumstances than those mentioned, illustrating its effects upon the discharging cardiac ganglia. When the pulse-rate falls very markedly under its use, as when it falls below 50, it would be well to substitute belladonna, squill, strychnia or casca for it.

P. 291: "In cases of right side dilatation, whether from mitral disease or lung changes, it is well to bear in mind the coexistent embarrassment of the respiratory centres, and to combine with digitalis, ammonia, nux vomica or belladonna. I will merely allude to other remedies stated to possess an allied action to digitalis. The *cereus Bonplandii*, introduced by Parke Davis: Lauder Brunton has found casca, the ordeal poison of Africa, to have a nearly similar action; Professor Frazer has used the *strophanthus hispidus*; M. Brandoun found the *dajask*, or arrow-

poison of Borneo, to kill with the heart firmly contracted in systole. These I am not familiar with in practice, but often have used the *scilla maritima*, as an excellent diuretic in cases of feeble pulse. The same can be said of *scoparium* or broom. Caffeine, though highly spoken of, I have not found to merit the laudation accorded it as a cardiac tonic, though I frequently found benefit in substituting belladonna when *digitalis* seemed, from unknown reasons, to fail me.

Convallaria majalis has been written up so recently in Penna. State Transactions by our member, Dr. Bruen, that I merely refer to that article.

My limited experience goes to show of *cactus grandiflorus*:

1. That it is a pure cardiac tonic, whether for functional or organic disturbances, especially in cases of mitral regurgitant disease.

2. *Convallaria*, though not of service in cases accompanying mitral regurgitation, appears, from Dr. Bruen's paper on the subject in the Transactions of the Medical Society of the State of Pennsylvania for 1883, to supplement *digitalis*, not to replace it; serviceable in backward distension of lungs, from mitral obstruction, a fine tonic for nervous and functional diastole of heart.

3. Belladonna and strychnia will frequently serve to substitute *digitalis*.

4. Caffeine citrate has been found to be of no effect in my experience for heart affections, functional or organic.

DOWNWARD DISPLACEMENT OF THE TRANSVERSE COLON. THREE CASES, WITH AUTOPSIES.

Read October 17, 1883.

BY CHARLES HERMON THOMAS, M. D.,
Surgeon to the Philadelphia Hospital.

A DEFORMITY of the transverse colon, consisting in the elongation of that portion of the large intestine and its displacement downward in the form of a loop or festoon, has been observed by me in three instances in private practice. Autopsies were had in them all. In the first the most dependent portion of the gut was found midway between the umbilicus and the pubic symphysis; in the second it was deeply impacted in the cavity of the pelvis; and in the third it reached the level of the umbilicus,

A positive diagnosis was not made in any of the cases, although in two of them the striking clinical conditions present were studied with special care in association with experienced and highly skilled observers. In the second in order of occurrence, the relationship between it and the preceding one suddenly occurred to my mind, and was communicated to the operator while on our way to make the post-mortem examination. In the third case the actual condition present was strongly suspected before death. So that in both of these latter, special precaution was used at the autopsies to avoid disturbing the relative position of the abdominal viscera until their location had been accurately determined.

The lesion here described seems to be of rare occurrence. Thus far I have failed to discover a single recorded case; and not until this paper was nearly completed was I able to find any published reference to the condition, however vague. Several months ago I asked the assistance of Dr. Formad, who informed me that in a series of autopsies, numbering over 2000, which he had made, he had not observed an instance of like character. He has also kindly sent me the following note:

"UNIVERSITY OF PENNSYLVANIA, Dec. 15, 1882.

"*Dear Dr. Thomas:*

"* * * I looked very thoroughly through the literature of intestinal lesions, but did not meet any record of misplacement of the transverse colon.

"Very truly yours,

"H. F. FORMAD."

CASE I.—Male, æt. eighty years, a retired gentleman, came under my care August, 1874, as a patient of Dr. J. J. Levick, who had placed his practice in my charge during his vacation, and who informs me that there was no previous history of abdominal disease.

The symptoms present were extreme emaciation, feebleness, anorexia, and a profuse but fitful diarrhœa. The abdomen was retracted and somewhat tender upon pressure. There was no complaint of pain except at intervals of three or four hours when the diarrhœa had ceased for a time. Coincidentally with the cessation of the diarrhœa, a tumor about five inches long and two inches wide, of firm consistency, and visible on inspection, appeared beneath the thinned abdominal walls in a transverse position midway between the umbilicus and the symphysis pubis. The tumor persisted but an hour or so at a time, disappearing immediately upon the return of the diarrhœa. During the periods of continuance of the tumor the pain was so severe as to rapidly weaken the patient. This condition of alternate flux and painful tumefaction was repeated several times daily until death took place. During the attendance upon the case there were associated with me Dr. Albert H. Smith and a distinguished physician

from another city—a near relative of the patient. With attention fully directed toward it, and after repeated observations, we were unable to frame a reasonable hypothesis as to the exact character and origin of the tumor. Death occurred September 12, about three weeks from date of attack.

Autopsy.—In the presence of Dr. Levick and the relative mentioned, I made the abdominal section. To the former I am especially indebted for the specimen obtained, and which is still preserved.

Upon laying open the abdominal cavity the transverse colon was found to be greatly elongated and proportionately narrowed, the loculi being nearly obliterated, forming a loop open at the top, similar to the letter U, the most dependent portion occupying the position of and constituting the tumor as above described, *i. e.* the horizontal portion of the loop rested upon the small intestines, midway between the umbilicus and the pubic symphysis.

CASE II.—Female, æt. fifty-four years, a lady of delicate frame and refined habits of life, was under my charge for about ten months prior to her decease. During the greater portion of this period Dr. Jas. H. Hutchinson was associated with me in the attendance. Dr. Chas. K. Mills also saw her for me during my vacation. The patient had previously been attended by a homœopathic practitioner who had diagnosed her condition as enlargement of the liver and stricture of the rectum. The latter supposed condition he had treated by the introduction of rectal bougies; this practice being afterward abandoned on account of the pain produced, and the lack of beneficial results.

Profound cerebrasthenia from other causes, with several months of delirium, and which finally led to a fatal result, served greatly to complicate the issues involved. The abdominal conditions which had been recognized from the beginning were thus either masked or placed entirely in abeyance during much of the time.

The more prominent symptoms recognized were (1) pain, referred chiefly to the region of the liver and extending both upward and downward, which pain was aggravated by walking, and was described as of a dragging, tearing character, and which had existed for four years or more. It was very much relieved by the recumbent posture, and after some months, spent mostly in bed, almost entirely vanished.

(2) Obstinate constipation, with indications of obstruction, even a liquid passage being voided with difficulty. The capacity of the rectum to retain enemata was also diminished to two ounces.

(3) Two solid tumors, elongated in form and of the consistency of solid fœces, were discovered, located one on each side of the abdomen, and evidently just beneath the parietal structures. They were vertical in position, and about eight inches distant from and so parallel to each other, and were traced from the border of the ribs to within about two inches of the pelvic brim. This condition was observed but a few times, and at considerable intervals; at other times it was absent. The hypothesis was adopted:

that these masses were the ascending and descending colon, respectively, in a state of faecal impaction.

Death occurred March 30, 1882; supervening upon a severe mental shock. An autopsy was made by Dr. Wm. M. Gray two days later, Dr. Hutchinson and myself being present. To quote from Dr. Gray's notes: "Upon opening the abdomen found complete prolapse of the transverse colon. It was carried beneath the pubis and rested on the bladder. The large intestine was much narrowed, and was filled throughout with hard nodulated faeces; the meso-colon was absent and the omentum, which was free from fat, was extremely atrophied; the rectum was normal, showing no evidence of stricture; the liver was of normal size, but upon microscopic examination showed marked cirrhosis."

Thus, that which had appeared to be the ascending colon proved to be the descending limb of the displaced transverse colon; and that which had seemed to be the descending colon was shown to be the ascending limb of the same malformation.

The pain which had previously been felt in the region of the liver, and which had been relieved by recumbency, had manifestly been caused by the sharp flexure of the colon contiguous to it; and the rectal obstruction by the crowded condition of the pelvis produced by the invading loop of large intestine.

CASE III.—Male, æt. thirty years, a tailor's cutter, under attendance nine days prior to decease. The subject of advanced Bright's disease, with "hyaline, epithelial and granular tube casts; also mucous cells, compound granule cells, and free oil globules," he was extremely exhausted thereby. He also complained of severe pain in the abdomen to the right of and slightly above the level of the umbilicus. Upon inspection and palpation of the part no enlargement or induration was discovered; but light percussion developed an intensely tympanitic sound confined to the region described. Misplacement of the transverse colon was suspected, and the region kept under observation for any evidences of faecal impaction which might, but which did not, present. Death occurred suddenly March 19, 1883. Autopsy two days later, by Dr. Wm. M. Gray, operator, Dr. Wm. H. Burke and myself being present.

The following notes were made by Dr. Burke. * * * "Body rather emaciated, and showing signs of commencing decomposition. On opening abdomen absence of fat noted, omentum normal. Peritoneum showing traces of lymph and pus, in the pelvic region especially, but no general inflammation. Transverse colon empty, distended with gas, and has a sharp flexure at its centre, bending obliquely downward and toward the right, to the level of the umbilicus, thence sharply upward to its normal position. Meso-colon intact and apparently normal except in length. No sign of faecal obstruction at the point of flexure. Both kidneys scirrhotic; capsule adherent, and secreting structure destroyed."

Evidently the heightened tympany localized near the umbilicus, which had been previously recognized and ascribed to the presence there of a portion of the transverse colon misplaced, had in reality been so caused.

No adhesions of the displaced parts were found in any of the cases cited. The intestinal fault was probably not the cause of death in any of them. Taking them together it will be seen that clinical conditions and post-mortem appearances agree in at least one important particular, viz.: the location of the displaced intestine in contact with the anterior abdominal wall and below its normal site.

The normal anatomical relations of the colon have a special significance in the light of these cases, from a diagnostic point of view. The ascending and descending portions of the colon are, normally, to be found in contact with the *posterior* or lumbar wall of the abdominal cavity—behind the small intestines—and are there bound closely down by reflections of the peritoneum. The transverse colon, on the contrary, is normally in contact with the *anterior* abdominal wall—in front of the small intestines—where it is loosely suspended by the transverse meso-colon; a structure of considerable length.

It therefore appears to be a practical impossibility for the vertical portions of the large intestine to become spontaneously misplaced anteriorly. But of the transverse colon, its displacement downwards—in which changed position its relation of contact with the anterior abdominal wall is retained—these cases show to be a condition of repeated occurrence.

Conclusions.—(1) Displacement of the transverse colon downward within the abdomen may be to any degree, partial or complete.

(2) Such displacement will present as solid tumor if the bowel be in a state of faecal impaction, or as a limited area of heightened resonance if the bowel be distended with gas; but in either case the displaced part is to be found *in contact with the anterior abdominal wall*.

(3) The occurrence of intra-abdominal tumor situated below the normal site of the transverse colon, and having the same general configuration as the colon, such tumor being of a certain consistency, and presenting evidences of being in contact with the anterior abdominal wall; or the occurrence of areas of special tympany with like outlines and similarly located, constitutes diagnostic signs strongly indicative of downward displacement of the transverse colon.

FURTHER NOTES ON THE USE OF HAMAMELIS IN
THE TREATMENT OF VARICOSE VEINS.

Read November 21, 1883.

BY J. H. MUSSEY, M. D.

SOME time ago the writer called the attention of the profession to the use of hamamelis in the treatment of varicose veins and their sequences.* Since then numerous inquiries have been made of him concerning this drug, and several cases have been reported to him of its use. It has, therefore, been deemed advisable to again refer to this plan of treatment in order to instigate further investigation by the profession, so that the exact value of the drug in this disease may be determined. In the first place, to determine this question, it is important to know whether the beneficial results of the treatment of the cases previously reported were permanent or not.

The three cases noted in full in this paper have been under my observation ever since that time. The first two may be dismissed at once by saying neither of them have had any return of the varicose veins or of any symptoms of them. Regarding the third, who was to be present to-night, it will be remembered that on account of the severity of his symptoms he was unable to work for nine months prior to having taken the medicine, and for three months of that time he was treated in a hospital by rest, pressure, etc. He returned to work two months after beginning the hamamelis, and has continued at his laborious occupation ever since. In answer to a summons, he presented himself two weeks ago. He had not taken any medicine for ten months. There was no return of any one symptom of his disease, save the varicosity noted below and slight œdema of the left leg. The tissues, however, readily take on ulcerative action, for every time a stone fell against his leg an ulcer formed, with this difference from formerly that it healed rapidly. On examination two inches below the knee, on the inner aspect of the leg, a congerie of veins are found. They are not painful, returned during the past month and have given him no trouble. The œdema of the ankle is not marked. There is a small healing ulcer on the right leg, which was caused by a stone falling on the leg a month ago. Both extremities are

* Phil. Med. Times, April 21, 1883.

very cold, on account of which he wears heavy stockings and woolen material—articles that were unbearable one year ago. When the past sufferings of this man are compared with the comfort and usefulness of the past year, in view of the previous systematic treatment of him, it can scarcely be gainsaid that hamamelis is of value in varicose disease. The writer takes pleasure in referring to his friend, Dr. Judd, who has been familiar with the case, past and present, and will substantiate the statements regarding him.

The subsequent experience of the writer has not been a large one, and only two cases can be referred to positively. Other cases were treated, but did not report for inspection after the second or third visit. Of the two cases, one was much benefited; the other not relieved. There is no sufficient cause for the failure of the drug in the last case. But in order not to present the facts alone of a probably prepossessed observer, the statements of numerous gentlemen will be given who have made use of the drug since the article referred to was published. It is, no doubt, natural that only the favorable cases have been reported to the writer—failures not being considered worthy of notice. There are some unfavorable comments given, however, and they will be first noticed.

Thus Dr. Dulles, surgeon to the dispensary of the University Hospital, writes as follows: "I tried it in a number of cases of leg-ulcer in the dispensary, and finally abandoned its use because I came to the conclusion that it was only of moderate value, and could in no sense be looked upon as a substitute for the ordinary surgical methods of treating these ulcers."

Rather more favorable is the testimony of Dr. Stelwagon, chief of the skin dispensary of the same hospital. He says: "The remedy was made use of in about fifteen cases—in patients with eczema, ulcers, or both, in whom the veins were at all enlarged. In three instances the results seemed, both to the patients and myself, favorable. In four or five cases the patients thought some benefit had ensued; I could not convince myself that such was really the fact. In the remaining cases, no improvement followed its use." In all the cases, the roller bandage was employed. Dr. S. says his experience is negatively favorable, and the remedy is worthy of more extended trial. Of the six cases treated by Dr. Van Harlingen, professor of skin diseases at the Polyclinic, there

was one quite successful case; two improved very much; the remainder made but two or three visits to the dispensary.

Still more favorable is the testimony to follow. Dr. R. M. Girvin reports two cases cured—no failures. One case, a female with varicosity of the deep veins of both legs, with swelling and induration of the limb and spots of ulceration as large as a dime. The veins were enlarged and tender; the pain was intolerable. One teaspoonful of the fluid extract of hamamelis every four hours was ordered, and improvement was seen in three days; a cure in two weeks. No other treatment was used, and the patient was on her feet most of the time during the treatment.

Dr. Shelly, of Ambler, Pa., reports the following:—

Female, æt. forty-five, cook, varicose veins in both lower extremities, of fifteen years' duration, unhealthy ulcer on outer aspect of left ankle-joint of ten years' duration. Ulcer followed a hemorrhage, and never healed. Both legs œdematous, the left much indurated. Eczema around the ulcer. Pains so great she has been confined to her chair or bed for one month. Treatment, hot bran baths and thin adhesive strips to left limb only; hamamelis in teaspoonful-doses five times daily. Relief almost marvelous, being about the house in one week, and three months afterwards the knotted and distorted veins had entirely disappeared, notwithstanding the continuance of her laborious duties. The rubber stocking which she had used for years was discarded, and its use has not been resorted to.

Dr. P. G. Skillern writes that he treated:—

Mrs. R., æt. fifty, for varicose disease of the veins of the left leg, with secondary œdema, causing pain and fatigue on exertion. September 17, ordered drachm-doses of the fluid extract, and the distillate externally. October 1, she reported much relieved, and October 15, cured, experiencing no pain from the limb.

Dr. Preston, resident physician to the Presbyterian Hospital, and Dr. Coddington, holding a similar position at St. Mary's Hospital, report cures. The former used it but once, and with success. Dr. Coddington used it in several cases, with satisfactory results, and recalls distinctly one case, that of a middle-aged man, who took drachm-doses of the preparation every two or three hours, with rapid and decided benefit.

To further illustrate the affinity this drug has for venous structures, Dr. Wm. E. Hughes writes that a case of phlebetis, secondary to chronic Bright's disease, was entirely and rapidly relieved by the use of this drug.

These reports ought to be of some avail to convince the most captious. It is thus seen that positive and negative results are given. The accuracy of observation cannot be doubted, and hence conclusions can only be vitiated by two factors, the preparation used and the dose exhibited. It is difficult to make a numerical statement, and so a definite impression only can be given to the effect, namely, that this drug is of decided value in a certain proportion of cases of varicose disease. If an estimate were to be made of the proportion of cures and failures, without fear of exaggerating, it may be said that one-fifth of all cases are cured, and that one-third of the remainder are benefited. Even with this small percentage in its favor, the inexpensiveness and simplicity of the plan behooves us all to try it. The testimony distinctly proves that the drug has a decided action on the veins.

The above allusion to the preparation is timely. One gentleman told the writer he found the drug of no use. On inquiry, it was found the distillate—a white preparation—was used. Another reported a negative result, but used a preparation whose value is doubtful. The fluid extract is the most reliable preparation—a dark one—and that from the laboratories of Bullock & Crenshaw, or Parke, Davis & Co., the strongest apparently. Regarding the dose, the writer feels that the one recommended in his former article is too small, and that if the amount or frequency were increased it would be valuable.

Other uses of the drug.—In addition to the above testimony, Dr. Girvin gives the notes of two cases of hæmaturia that were cured by the exhibition of hamamelis:—

1. Mrs. P., æt. forty; hæmaturia; for six weeks had passed blood every day; some days in large quantities. She was anæmic, weak, without appetite, and rapidly failing. Test of urine showed albumen in large quantity, which Dr. Formad decided was due to the blood. Ordered thirty-drop doses of the liquid extract every three hours. The third day the bleeding ceased and had not returned two months after the first administration of the drug. No albumen could then be found in the urine.

2. Mr. P., æt. fifty-three, was relieved promptly of the same disease. "I have used it," he says, "in a number of cases of menorrhagia, and regard it as more certain in its effects than ergot or any other styptic in use for such conditions."

Dr. Dulles adds to his communication that he considers hamamelis a good astringent and a stomachic tonic.

In conclusion, the writer desires to express his indebtedness to the many gentlemen for the privilege of using their notes.*

3706 POWELTON AVENUE.

DISCUSSION ON USE OF HAMAMELIS.

Dr. O'Hara: I think that, to form a judgment of the value of this remedy, we should try it on old cases. If it is used internally, it probably acts through the nervous system.

Dr. John B. Roberts: I think we should divide cases of varicose veins into two classes, according to the duration of the condition. In the early stages we probably have a vaso-motor paresis of the vascular wall. In this, internal remedies may possibly be beneficial, but not in the long-established cases, where the valves and vein-walls are greatly altered. I cannot see how internal treatment alone could do good, when the stretching is constantly increased by the hydrostatic pressure of a long column of blood. The cases detailed by Dr. Musser were also treated by bandaging, so that the actual effect of hamamelis is left somewhat in doubt. I can, however, see that in the condition of subacute phlebitis, which arises in varicose veins, and often first causes the patient to consult the surgeon, hamamelis might be valuable, especially if combined with the rest from work which is apt to be taken at such times. Dr. Girvin, according to the paper, reports the successful use of the remedy in phlebitis, which tends to confirm the thought that I had as Dr. Musser was reading the early part of his communication.

Dr. Collins: I think that almost all cases may be successfully treated, if we can remove the cause. We should get rid of the elastic garter; this is a frequent cause. Recent cases may yield to treatment, but old cases will not so easily. I have used the remedy in question successfully, in cases of hæmorrhoids.

Dr. Bartholow: Some time ago I tried the proprietary preparation of hamamelis, but found it without effect when given, even in large doses, to the lower animals. The officinal fluid extract is an active preparation, and will be found of use in hæmorrhoids and varicose veins when of recent development, the muscular coat being in a paretic state. The active ingre-

* Since reading the above, Dr. Randall, resident physician at the Phila. Hospital, reported as follows: One case of varicose disease not benefited. A second case of the same, with secondary ulcers and œdema, was relieved in three weeks. Treatment discontinued, a relapse followed; treatment renewed without benefit. In the third case the disease was of three years' duration, in a man æt. fifty-two. He had tried numerous remedies without benefit. The use of hamamelis and a roller bandage gave him decided relief. Finally he reports that D. K., æt. forty-eight, a sufferer from "bleeding piles," had been cured in three weeks with drachm-doses four times daily of the drug.

dients of the drug are tannic and gallic acids, and in these rests the utility of hamamelis. It is well known that these acids arrest hemorrhage, and contract vessels.

Dr. Trautmann : I have tried the remedy in four cases. In one case the patient had been under the care of a number of physicians, and was at the time of treatment unable to stand on her feet. This was a chronic case, and led to eczema and ulcerations. Hamamelis gave good results, but after some months the veins were again distended; the condition, however, was again controlled by hamamelis. In the second case I used the remedy in conjunction with the usual rubber bandage, also successfully. The third case I lost sight of, and in the fourth case a cure was obtained.

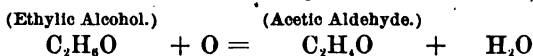
NOTE ON PARALDEHYDE AS A HYPNOTIC.

Read November 21, 1888.

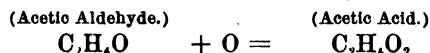
BY J. C. WILSON, M. D.

PARALDEHYDE has during the past year been made the subject of occasional contributions from various sources to the journals. Its introduction as a drug is due to the Italians, and especially to Cervello, of Palermo, and Morselli, of Turin. It is, above 50° F., a colorless, limpid liquid, of a specific gravity of .998, boiling at above 225° F., and soluble in about eight parts of water at 52° F.

Chemically the Aldehydes are bodies obtained by limited oxidation of alcohols, from each molecule of which two atoms of hydrogen are eliminated with the production of water, thus :—

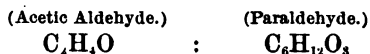


By further oxidation acids are produced, and these correspond in composition with the alcohols whence they are derived, thus :—



In the presence of nascent hydrogen, however, aldehydes again take up their lost atoms of hydrogen and become alcohols.

Paraldehyde is formed by the action of certain acids, *e. g.* sulphuric, hydrochloric, sulphurous, etc., on acetic aldehyde at the ordinary temperature; it is a crystalline body below 50° F., and is a polymer of acetic aldehyde; that is, its percentage composition is similar, but its molecule is a multiple of that substance, viz. :—



I am indebted for information concerning this substance to the *Medical News* (July 28, and Oct. 20, 1883), to Dr. C. L. Dana's communication in a recent number of the *Medical Record*, and to Mr. Genois, of Messrs. Wyeth & Bro., from whom the specimens I have used were obtained.

The medicinal dose is from thirty minims to two fluid-drachms. I have found a drachm to be the average dose for an adult under ordinary circumstances. It is to most patients disagreeable and must be administered with a considerable draught of water. The taste and odor are ethereal and penetrating. Patients complain of this taste several hours after taking it, and it may be recognized by its odor in the breath. It is probably eliminated unchanged by way of the lungs.

Paraldehyde acts upon the cerebral hemispheres, inducing rather speedy drowsiness without preliminary excitement. "A lethal dose suspends the functions of the medulla and the respiratory centre, and the action of the heart ceases after the respiration." One observer (Brown) noted a slight depressant effect upon the heart in a single instance. It is stated that neither nausea, depression, headache, constipation, nor any unpleasant after-effects have followed its administration. Several of my own cases complained of the disagreeable after-taste already alluded to, and one or two of nausea.

Dr. Dana gave a pup six months old a gramme by the mouth. "The animal was at first much excited, running around and stumbling as if intoxicated. It showed no signs of pain or gastric disturbance. Its intelligence was not greatly disturbed; it came when called. Pulse ran up from 130 to 200; respiration was 20 to 24 and labored. In about twenty minutes, it lay down and went to sleep. Pulse 140; respiration slower (18), and with labored inspiration. The animal was easily roused, walked around, then went to sleep again. Slept about two hours." Cervello has recently demonstrated a direct antagonism between paraldehyde and strychnia, the former diminishing the reflex excitability of the gray matter of the medulla oblongata, whilst strychnia increases it.

Paraldehyde has been prescribed as a hypnotic by the Italian physicians who have used it, in the various conditions calling for such a remedy, but they have found it especially serviceable in the

sleeplessness of dementia paralytica, hysteria, and in other forms of disorder of the nervous system.

Dana employed it in doses not exceeding three grammes in a number of cases. In six cases it acted well as a hypnotic; in two it was helpful; in one it failed. Temporary relief of pain followed its administration in sciatica, and supra-orbital neuralgia.

I have prescribed it in nine cases, with a view to its influence as a pure hypnotic.

In one hysterical patient it acted well for a short time, but lost its effect, and was discontinued. In a patient who could not sleep, after having acquired the habit of watching an invalid at night, it procured prompt and refreshing sleep. In a lady rendered sleepless by a sudden and appalling bereavement, it caused sleep, but was abandoned on account of the nausea which followed its administration. A gentleman who had sleeplessness and great mental depression, after a debauch, and who failed to sleep for several nights after reasonable doses of the bromides and chloral, took a drachm of paraldehyde, and slept seven hours, waking refreshed and hungry. On the next day, this patient, being disturbed after he had taken it, failed to sleep, but succeeded in sleeping on taking a second dose. The other cases were sleeplessness from ordinary causes, and were all more or less fully relieved. It appears to speedily require an increase of the dose.

If I may venture to express a personal view, it is that paraldehyde will prove a useful addition to our sleep-inducing drugs, but will supersede neither chloral, which it resembles in its effects, nor any others among them.

It is, like new products of the chemical laboratory, at present expensive. There is no reason why a demand for it should not cheapen it.

I thank Dr. O. Horwitz, resident physician in the Jefferson Hospital, for assistance in observing such of the cases as were treated in that institution.

DISCUSSION ON PARALDEHYDE.

Dr. Bartholow said: I have had some experience with this remedy, and I think Dr. Wilson's judgment of it is well founded. It will not take the place of chloral, because it is not so certain in its action. It does not, however, depress the heart, and its action is not extended to the medulla unless large doses are taken. We cannot accept unreservedly the state-

ments of the Italian physicians. We cannot forget the claims which Prof. Polli made for the sulphites. As far as my own experience goes, I have found the remedy not to produce excitement, except in a single case. In another case it failed to produce sleep when pain existed. It has, therefore, a limited value. In those cases in which the difficulty is simply an inability to go to sleep, and no active cause of sleeplessness exists, the drug is applicable.

Dr. Wilson said : Of these cases, the one in which paraldehyde appeared to have the best and promptest hypnotic effect was an individual in whom the conditions closely corresponding to the state described by Dr. Bartholow existed. This man was prevented from sleep by the mental state of remorse and chagrin which supervened upon his recovery from a disgraceful debauch. Other hypnotics in reasonable doses had failed.

PROOF THAT HUMAN MILK CONTAINS ONLY ABOUT ONE PER CENT. OF CASEIN; WITH REMARKS UPON INFANT FEEDING.

Read December 12, 1883.

BY ARTHUR V. MEIGS, M. D.,

Physician to the Pennsylvania Hospital and Children's Hospital.

YOU must give me your indulgence for bringing before you a subject necessarily somewhat dry in its details, but its great importance must be my justification. Nearly two years ago, I had the pleasure of reading here a paper upon "Milk Analysis," when I described the method I had worked out, and briefly detailed my results. The main conclusion was that human milk never contains more than about 1 per cent. of casein, and this observation I then claimed to be a new one, and very important, as showing how we should feed infants so unfortunate as to be deprived of their natural sustenance. A year ago or more, I delivered a lecture to the class at the Pennsylvania Hospital, which was subsequently published in the *Medical News* of November 4, 1882, in which I described a plan of feeding deduced from the results of numerous experiments and analyses. This evening, I wish, first, to offer proof that the results arrived at by the method of analysis are correct, and, second, to make some suggestions about the proper method of infant feeding.

Although many chemists have made analyses of human milk, and a great variety of divergent results have been attained by different methods, there has as yet been no proof offered of the correct-

ness of any of them. This constitutes an important missing link in any attempt to place the question of the composition of milk upon a settled basis; and if a method of analysis is ever devised that will give results which shall be universally accepted, and stand the test of time, the accuracy and correctness of the method must be susceptible of proof—simple, scientific and incontrovertible proof. Some proof of the correctness of the analyses described here two years ago will now be attempted.

No one disputes that in ether, chemists have a perfect solvent for fat, which, when properly applied in milk analysis, extracts it all. The fat when separated can be seen, and the eye tells positively that it is fat.

With regard to the water, it is equally certain that by the evaporation process its amount can be accurately estimated. This statement is made with the knowledge that there may be with the water some slight traces of other fluids—alcohol, for instance, and perhaps other volatile liquids; but these must be in such minute quantity that they need not be taken into consideration, and for the present the liquid portion of milk may be spoken of as the water. It is equally certain that in incineration, properly performed, there exists an easy and correct method of determining the amount of inorganic matter. In the future, of course, there may be perfected some way of estimating the salts in milk, by extracting from the liquid milk, or from the solid residue left after evaporation, and this may show them to exist in larger quantity than the present method of incineration leads to believe; but the possible error introduced in this way must be very small, and does not invalidate the general facts stated. That the existing estimates of the water, fat and inorganic matter are correct, is further proved by the fact that there is no difference of opinion in regard to their amounts. Examination of the analyses of different chemists shows an almost exact uniformity of conclusion with regard to the relative quantities of the above-mentioned substances.

When the estimates of the casein and sugar, however, are considered, the widest divergence of view is discovered in the conclusions as to the amounts existing in human milk. In regard to cows' milk, chemists all arrive at nearly uniform conclusions. The casein in human milk is estimated by Dolan and Wood, in one of their analyses, at 7.005 per cent., Vernois and Becquerel

give it as 3.924 per cent., and Henri and Chevallier as 1.52 per cent., while my own experiments lead me to conclude that there is about 1 per cent. Now the fact is a striking one, that if in any of these analyses the sugar and casein amounts be added together, the sums are found to be in each instance nearly the same. The subjoined table shows this to be the case.

	Vernois and Bequerel.	Simon.	Henri and Chevallier.	Dolan and Wood.	Haidlen.	L'Héritier.	Doyère.	Clemm.	Tidy.	Meigs.	Payen.	Quevenne.	Regnault.
Casein	3.924	3.43	1.52	7.005	3.1	1.30	.85	3.533	3.533	1.046	.215	1.05	3.9
Sugar	4.364	4.82	6.50	1.921	4.3	7.80	7.31	4.118	4.624	7.407	8.805	7.31	4.9
Total	8.288	8.25	8.02	8.926	7.4	9.10	8.16	7.651	8.157	8.453	9.020	8.36	8.8

The estimates of Haidlen, L'Héritier, Doyère and Clemm are taken from a table in the *Physiologische Chemie* of Gorup-Besanez; those of Vernois and Bequerel, Payen and Regnault, are taken from one in the *Traité de Chimie Pathologique* par Bequerel et Rodier; but the others are from the original sources. A complete estimate of all the constituents is not attempted in the analysis of Quevenne; and under the head which is called casein in the table is included albumen (*matière albumineuse précipitée par l'alcool*); under that of sugar are included also extractive matters (*lactine et matières extractives*).

The table also shows that in each instance where the casein amount is large, the sugar is small, and *vice versa*—that where the casein amount is small, that of sugar is large.

It has been already said that as regards the analysis of human milk, all observers are agreed as to the proportions of the water, fat, and ash; it is now further evident, from the table, that all agree as to the quantities of casein and sugar taken collectively, and that only when the separation of the two is attempted does there exist any difference of opinion. The separation of the casein from the sugar, therefore, is the difficult part of milk analysis, for in regard to this alone is there any difference of opinion. In this portion of milk analysis then is reached the stumbling block, and it alone requires any explanation, for the rest is universally conceded, and cannot, therefore, but be considered as already placed upon a scientifically exact basis.

There are but two possible explanations of the different results arrived at by various investigators; one, that human milk is as variable a substance in regard to the amounts of casein and sugar contained, as the different analyses would lead to believe; and the other, that the methods of the majority of chemists have been faulty and their conclusions incorrect. That the second of these

two explanations is the correct one, does not admit of doubt. Wanklyn says that cows' milk is a substance exhibiting great uniformity of composition, and what he says of cows' milk is also probably true of human milk. There is no reason to expect that it would vary so much in regard to the proportions of casein and sugar, when cows' milk exhibits such uniformity of composition in these respects. It may, by analogy, be fairly argued that human milk is very unlikely to be so variable as published analyses would seem to show. The proof of this, however, lies in showing, by examination of a large number of specimens, that human milk always contains a large amount of sugar (say 7 per cent.), and therefore by exclusion it cannot contain the great amount of casein it is usually credited with, for all observers agree as to the sum of the amounts of the two substances.

The existence of this large amount of sugar in human milk I have endeavored to demonstrate by experimenting as to how much could be obtained in the crystalline form from any fixed quantity, and then, by applying the same process to cows' milk, to find out whether an equal or, as should be the case if my already published original analyses are correct, only a less quantity of sugar will take the crystalline form.

An experiment was made as follows: 10 c. c. of human milk, which had already, by the process described in my previous paper, been found to contain 7.224 per cent. of sugar, was, as usual, agitated with ether and alcohol, and the fat removed. After the removal of the fat the remaining portion was carefully washed into a dish, and in the water-bath, at a temperature of 70° to 80° C., evaporated until only about 10 c. c. of fluid remained; upon this was poured a mixture of 25 c. c. of water with 25 c. c. of alcohol, and the whole allowed to stand over night. By morning a precipitate had formed and settled to the bottom of the vessel; this was thrown upon a filter and washed with a mixture of equal parts of boiling alcohol and water. The filtrate was again reduced in the water-bath, at a temperature of 70° to 80° C., to about 10 c. c. and then 75 c. c. of absolute alcohol added. This caused again the formation of a slight precipitate, which was allowed to thoroughly settle to the bottom, when the perfectly clear fluid above was poured off into a dish, care being taken that none of the precipitate passed over with the clear fluid. This liquid was allowed to evaporate, without heat, in an open dish of known

weight, and there remained finally, only crystalline milk-sugar, with a very minute amount of the inorganic material. The 10 c. c. of milk thus treated, yielded 659 milligrammes of sugar dry at 100° C. This milk had been previously ascertained to contain 738 milligrammes of sugar to each 10 c. c., which made its percentage of sugar 7.224, as already stated. These figures are sufficiently nearly parallel to prove the point, for by the method of crystallization described, it was not expected that all the sugar present would be obtained in the crystalline form, some of it being necessarily precipitated with the casein in the course of the manipulations with alcohol; but only to obtain it pure, and in sufficient quantity, to prove that the sugar existed in the milk in the large quantity shown by the analysis, and therefore necessarily by exclusion, the existence of only the small amount of casein. The demonstration thus obtained seems incontrovertible, for when the existence of the large amount of sugar is shown, it follows as a necessary corollary that there can be only the small amount of casein, and therefore the existence of the small amount of casein in human milk is proved.

As a means of further proving that the sugar obtained by crystallization, as described, was entirely free from any traces of casein, it was tested for me by Mr. J. K. Hecker, the apothecary of the Pennsylvania Hospital, by the Nessler test described by Wanklyn and Chapman (*Water Analysis*, by J. Alfred Wanklyn and Ernest Theophrong Chapman, London, 1876, p. 25). This test decomposes the casein and forms ammonia from the nitrogen. When the crystalline sugar was subjected to its action, it showed it to be practically free from casein.

Cows' milk, when subjected to the same process of precipitation of the casein by alcohol, after the removal of the fat, yielded only about 4 or 5 per cent. of crystalline sugar. The manipulations were not carried out with the care that was taken when human milk was examined, for there is no dispute as to the amount of sugar in cows' milk; the experiment was therefore made merely to afford confirmatory evidence of what was shown by that upon the human milk. For if only a little more than 4 per cent. of sugar existed in human milk, as is claimed by Vernois and Becquerel, and others, and this being the quantity universally conceded to exist in cows' milk, then when both were subjected to the action of the same reagents, the same amount

only of crystalline sugar should be yielded. This, however, was not the case.

One of the strongest proofs of the correctness of the estimates of fat in milk is afforded by the fact that after it has been separated, it can be seen, and the eye tells that it is fat. When sugar is crystallized, and can be seen and felt, and examined with the microscope or a magnifying-glass, and the characteristically shaped crystals of milk-sugar are seen, the fact that it is sugar, and nothing else, becomes self-evident.

To test still further the accuracy of the method described in my former paper upon milk analysis, I carefully analyzed a specimen of human milk and found it contained the different proximate constituents in about the usual quantities. Then I separated from a further portion of the milk, taken at the same time and under exactly parallel circumstances, fresh portions of the casein and sugar, which I gave to Mr. Hecker, to test their purity; the sugar to be subjected to the Nessler test, to discover if it contained any casein, and the casein to be subjected to the action of Fehling's test, to find whether or not it was free from all traces of sugar. The casein entirely failed to produce any effect upon the copper solution, thus showing that it was free from sugar, while if a small portion of the sugar was added to the solution, the characteristic reduction of the copper at once took place. When the sugar was subjected to the Nessler test, .05 grammes being introduced into the retort when the decomposing materials were ready; it almost entirely failed to react, showing no more change than would be accounted for by the distilled water which had been used to prepare the sugar. This distilled water must have contained traces of organic matter, for when it was subjected to the test it showed slight traces of ammonia. The test is so delicate, that it is only by the greatest care and nicety in preparing the materials that they can be had perfectly free from all nitrogenized materials. The conclusion was that the sugars—both that prepared by the ordinary process advised for analytic purposes, and that obtained by crystallization—were, practically speaking, free from casein.

If, then, it has been shown that human milk contains approximately 87.1 per cent. of water, 4.2 per cent. of fat, 7.4 per cent. of sugar and 0.1 per cent. of inorganic matter, the proof that it

contains, not 3 or 4 per cent. of casein, as is commonly taught, but only about 1 per cent., is complete.

It may seem bold to make statements directly contradictory of the correctness of many of the usually accepted standards, but it is done with the full knowledge that it is liable to contradiction. There is an article on Infant Foods, by Prof. Albert R. Leeds, in the Transactions of the College of Physicians, third series, vol. vi, Philadelphia, 1883, in which he gives results quite different from my own; but as he gives no account of the methods he pursued, any criticism of his results is at the present time impossible.

In the endeavor to find a food which shall be the best for infants who have to be hand-fed, there are two considerations, either of which might be selected as the basis from which to start. In the first place, the desired goal might be attained by making trial of all sorts of foods, and these being put to the test of experience, the good would be retained and the bad gradually weeded out, until at last perhaps the most suitable would be found, and slowly introduced. On the other hand, the desired end might be attained by trying to produce a food which should be, as nearly as possible, like what nature has provided for the infant. Many trials have been made in the past by both these methods, but to the second one justice has never been done; for, if my conclusions are correct, a proper understanding of the composition of human milk, from which to start, has been wanting.

A clear understanding being now had of its proximate constituents, and the proportions in which they exist, it is possible more intelligently to set about finding how the same elements may be had, and mixed together to make an artificial food like human milk. Cows' milk is almost universally the basis of the foods used, in this country at least.

The artificial food which I shall presently recommend is the outcome of a study of the subject from both of the standpoints suggested, and its advantages are demonstrable. Upon theoretical grounds, it is what a food for infants should be; for analysis of human milk and cows' milk has shown what is their composition, and in the artificial food the elements have been introduced in the same proportions as they exist in human milk. Experience has for many years past been tending in the direction of proving such a food to be what is needed; for, while almost innumerable manufactured infant foods of every variety have been introduced,

and have often for a time been thought all that could be desired, they have all, one after another, fallen into disuse and been forgotten; but the use of cows' milk continues to hold its own, and in civilized countries is employed ten times more than all the manufactured foods together. The question, however, remains of how to use it, and the various methods suggested have been almost as numerous as the physicians who have advised them. For a long time the great majority of writers upon infant diseases and diet have recommended that cows' milk should be diluted before giving it to young infants; and this, they all agree, is because it contains too much casein, which causes a curd that only infants of the strongest digestion can with safety assimilate.

The weight of testimony that cows' milk contains much more casein than human milk, is so great that it is astonishing how almost universally the analyses of human milk of Vernois and Becquerel, and of those who have arrived at like conclusions, have been accepted and given credence, in despite of the fact that the evidence of the senses of every one who has examined into the matter is diametrically opposed to such an acceptance. Although, as already said, the weight of authority has long been in favor of the use of diluted milk, still there have always been those who recommended it to be used pure. Of later years more and more has been said and written upon the advantages to be derived from the use of cream, or diluted cream. Dr. J. Forsyth Meigs, who was a well-known authority upon the complaints of children, for years used with great success a mixture of equal parts of milk, cream, lime-water, and a weak arrowroot-water, with a little sugar. Cream mixed with whey, to increase the sugar and lessen the amount of casein, has been recommended. Biedert (Virchow's Archiv, Band 60, 1874) has written an article, and concludes that the best food is cream and water, one part to four, with 15 grammes of milk-sugar to the half litre of the mixture, the strength of this to be gradually increased. Biedert made many experiments comparing the relative coagulability of human and cows' milk, and again the digestibility of the coagulum; as can therefore be pre-supposed, his experiments turned mainly upon the two kinds of casein and their differences. He concludes that "there are two important points in which human and cows' milk are unlike: first, in the different amounts of casein contained; and second, in the absolute chemical difference of the two sorts of casein. The first

of these considerations would be of but little importance, however, if the analyses which place the average of casein in human milk at 4 per cent. are correct; its importance, on the other hand, would be very great if it usually contains—as I, in agreement with Vierordt's view, believe—only from 2 to $2\frac{1}{2}$ per cent. I think many further analyses are necessary to establish absolutely this point. Even if this view is accepted, however, dilution of cows' milk with equal or more parts of water is not sufficient to remove the differences. It is well known that such a dilution does not remove all the disadvantages which arise in the use of cows' milk, and my clinical experience has taught me that even dilution with two parts of water does not attain the desired end; and the explanation of this positive irremovable difference is to be found in the important chemical differences which exist, the casein of cows' milk coagulating so much more easily, and the coagulum being so much more firm than is the case with the casein of human milk; and, on the other hand, the coagulum being so much more difficult of solution or digestion.

“Until we succeed in actually making the casein of cows' milk identical with that of human milk, it will be necessary to give infants only so much of it as they can digest (no matter how great the necessary dilution may be), and to make up to them with carbo-hydrates (fat and milk-sugar), the lack of albuminates in the food.” He further says: “After numerous experiments I have come to the conclusion that the amount of cow casein which an infant's food should contain is 1 per cent. The fat and sugar in cows' milk appear to be as easily digested, and in no wise different from those contained in human milk. If, therefore, one-eighth of a litre of sweet cream (which, according to Hoppe, contains $9\frac{1}{2}$ per cent. of fat, 3 per cent. of sugar and 4 per cent. of casein) is diluted with three-eighths of a litre of water, which has been previously boiled, and milk-sugar is added in the proportion of 15 grammes to the half litre, the desired cream mixture is produced, and contains 1 per cent. of casein, 2·4 per cent. of fat, and 3·6 per cent. of milk-sugar, which will be found, under all circumstances, to be well borne, and is a sufficiently nourishing food.” The greatest part of Biedert's admirable article consists of a detail of experiments made of treating cows' and human milk, and the caseins obtained from both sorts, with a variety of reagents, and observing the different relative effects produced. His conclusion is that “the pure casein of human milk is, in both

its physical and chemical nature, different from that of cows' milk." The casein of cows' milk, when isolated, has always an acid reaction, while, on the contrary, that obtained from human milk is always alkaline. If human casein is treated in a certain way with acid, there is produced an "acid modification of human casein," which has many points of resemblance with ordinary cow casein; on the other hand, by treating cow casein with alkali, a substance is produced which shows, with many reagents, identically the same changes that are, by like treatment, produced in human milk. After careful examination of these two substances, however, Biedert concludes that "cow casein treated with alkali is, in many respects, much more like human casein than the original cow casein, yet it always shows unmistakable differences." Although he makes a strong case, and there are many reasons in favor of accepting his conclusions, yet, in the present state of knowledge of casein, the difference cannot be considered as absolutely demonstrated.

There are objections to such a belief; it has been already shown that human milk contains only one-third the amount of casein that exists in cows' milk; and there is a further important difference, which Biedert also appreciates, that human milk is always alkaline, while, on the contrary, cows' milk is acid. A coagulum, therefore, produced in a solution which is relatively so concentrated as is the case in cows' milk, and further in a fluid which is acid in reaction, is a very much denser and larger one than can be had from the relatively weak solution in human milk; and it is quite possible, therefore, that the difference may be owing to the different degrees of concentration, and the difference of the fluid media in which the casein is held; it cannot, therefore, yet be conceded that Biedert has absolutely demonstrated that the two caseins are chemically and physically different, although he has brought many strong arguments to bear. It is impossible to decide with certainty about casein in all its relations, while as yet it is not even known whether it is a simple or compound substance. Its solubility or insolubility after it has once been precipitated depends in great part upon how the original coagulation was effected, and whether or not it was thoroughly dried. If casein is once thoroughly dried for a good many hours at 100° C., it becomes absolutely insoluble in water, and will not dissolve even in a strong solution of caustic soda. Lehman (*Physiological Chemistry*, Cavendish Society Translation, vol. i, p. 378) says:

"I believe that the jelly-like coagula of women's milk are more dependent on the alkaline state of the fluid than on any peculiarity in the casein; at all events, I have found that women's milk, when acid, yields a much thicker coagulum than when alkaline, and cows' milk, when alkaline, a much looser coagulum than when acid—facts of the highest interest and value in relation to dietetics."

Whatever may finally be decided about casein—whether those of cows' and human milk are as different as Biedert believes he has proved, or whether they are nearly alike, the difference being merely that the quantities are not the same and the containing fluid media different—what most concerns the subject in hand is the relatively small quantity which exists in human milk; for it shows conclusively that in a food for infants, the amount of casein in cows' milk must by some means be reduced to equal the amount in human milk. The correct conclusion of Biedert, that not more than 1 per cent. of cow casein should be present in a food for infants, is the more surprising as he arrives at the opinion from a totally different reason from the true one that human milk contains only 1 per cent.

Although Biedert's conclusions are very instructive, as he arrives at them from clinical experience, and surprisingly correct in many respects, he goes astray in assuming an incorrect standard of the average composition of cream. The estimate of Hoppe, which he assumes to be correct, places the amount of casein too high; for, as may be seen by a reference to my table, cows' milk and cream do not contain more casein than sugar. The usual estimates rate the casein too high, at the expense of the sugar. This being the case, and Biedert reckoning the composition of his cream mixture from this incorrect standard, and not from any analysis either of the cream or the mixture itself, as should have been done, places his fat amount too low, as only a very poor cream is so weak in fat as his standard rates it. A mixture made as he directs is far weaker in sugar than human milk, and therefore, although perhaps proper for temporary use in cases of indigestion, cannot be accepted as a standard of what an infant food should be; and it entirely fails to accomplish what he says should be done—make up to the infant by an excess of carbo-hydrates the lack of albuminates which exists in the food—for it only contains about half as much sugar as exists in human milk.

COMPOSITION OF HUMAN MILK.

Amount of Fat in Each.	Creams.	
	No. 1	No. 2
19-020	1	2
17-507	1	2
13-382	3	3
12-470	4	4
17-129	5	5
16-092	6	6
13-825	7	7
14-950	8	8
18-082	9	9
16-552	10	10
12-159	11	11
15-611	12	12
19-071	13	13
11-782	14	14
18-519	15	15
21-465	16	16
21-200	17	17
Average from the 17.....		
16-308		

The human milk average is the result of ten analyses made. Eight separate analyses were made of the milk of different women; on another occasion, equal quantities of milk were taken from twenty-seven (27) women, and a portion of this analyzed; on a third, equal quantities of milk were taken from eight (8) colored women, and this subjected to analysis.

All these facts show that the tendency has been constantly toward the truth, and that physicians have been learning empirically for what reasons cows' milk has failed as an infant food, and how the difficulties which its use entailed were to be overcome. The use of cream has been advised; cream and whey; diluted milk; diluted milk with milk-sugar; cream, milk, lime-water and arrowroot-water; and finally comes Biedert's cream mixture, and he arrives more nearly at the true solution of the difficulty than any of the others, but still falls wide of the mark, from want of a precise knowledge of the composition of human milk, and of cows' milk and cream.

Investigators have thus, year by year, and step by step, been approaching the desired goal, and it needed but a touch for light to be let in upon the whole subject. Many hours and much careful and patient labor have been expended in investigations in this field, and no single worker could have done his part without having the results of the labors of his predecessors before him, to guide him a long way in the field, and give him easily the knowledge which would enable him, after much toil and trouble, to advance one little step more towards what was previously unknown. Thus, no individual investigator, no matter how important the advance in knowledge he may have made, should assume too large a share of credit; for it can be but a very small part of the great whole, and would be valueless but for the rest, into which it fits, and completes that which would otherwise be useless.

The necessary data being now at hand, it is comparatively easy to construct a food which shall, at least, be more nearly what is needed than any previous one. In making such a food, there are two matters to be considered: the proximate constituents must be in the same relative proportions as they are found in human milk, and they must be in a medium which shall be, as human milk is, alkaline. This latter end is easily accomplished by the use of a due amount of lime-water, and is justified by the fact that it is a matter of experience, almost universally acknowledged as true, that it is a most useful adjunct, rendering cows' milk more easy of digestion by the human stomach. The quantity of lime-water to be used should be one-fourth of the total by measure. This may seem to many persons an excessive quantity, but when it is understood that if made as ordinarily

directed, by agitating water with lime and then filtering, it contains only two decigrammes of lime in each litre, it becomes plain that the use of lime-water means the administration of a great deal of water and very little solid matter. The above estimate was arrived at by direct experiment; 10 c. c. of freshly made, filtered lime-water, being evaporated, was found to yield 2 milligrammes of lime. This is a very large estimate of the amount of lime soluble in water, as may be seen by reference to the U. S. Dispensatory, or the National Dispensatory, both giving it as much less. That the use of lime-water (alkali) in an infant food makes a difference in its behavior with some reagents is shown by the following experiments. A food was made in the proportions which will presently be given, and 10 c. c. of it agitated with ether and alcohol, as directed in my previous paper read before the Society, for the extraction of the fat; it was found that the coagulation took place in the form of a fine network, which remained permanently distributed through the lower stratum of the liquid, no sediment forming at the bottom. When an exactly similar mixture was made, except that the lime-water was replaced with water, leaving the fluid acid, and this agitated with ether and alcohol, thick, heavy curds formed, which at once sank to the bottom. Again, when two mixtures—one with and the other without lime-water—were treated with 10 drops of acetic acid, the one without lime-water showed much larger, heavier coagula than that which contained lime-water. These experiments show with certainty that the addition of lime-water does alter the coagulability of the casein when experimented with, whatever may take place in the stomach; and I have already quoted Lehman's opinion that the acidity or alkalinity of milk makes a difference in the formation of the coagulum. Whatever may be the value of these artificial experiments, the great reason for the use of lime-water is that the experience of man has found it good, and that is sufficient reason for its use in the present state of knowledge. It is quite possible that in the future something better may be found, phosphate of lime perhaps, for it is the salt which exists in milk in larger quantity than any other; but further and exhaustive study of the inorganic constituents of both human and cows' milk will be required to place this matter upon an exact scientific basis. It is very desirable that further study of the salts of milk should be prosecuted, and

it is much to be hoped that in the near future exhaustive analyses will be made. The amount of inorganic matter in cows' milk is so much greater than that in human milk that, as there is at present no means of removing it without altering or destroying the other component parts, no infant food can be made exactly like human milk in respect to the amount of salts contained.

So far as bringing the other proximate constituents to like proportions with those in human milk, the first step must be to so dilute with water as to get the desired quantity of casein; the fat and sugar can be increased by the use of the necessary quantities of cream and commercial milk-sugar. Taking the averages of cream and good city milk as already given (see table), it will be found by calculation that if there be mixed together 10 c. c. of cream, 5 c. c. of milk, 10 c. c. of lime-water, and 15 c. c. of water, with 2.2 grammes of milk-sugar, the desired mixture is had. That this is the case should not be trusted to mere calculation, but an analysis of the mixture should be made, both to verify the calculation and to observe how the mixture behaves when subjected to the analytic processes, whether it in its reactions more closely resembles cows' milk, with which it is made, or human milk. The table shows the results obtained by such analyses.

The easiest way to prepare and use the food is as follows: there must be obtained from a reliable druggist packages of pure milk-sugar containing seventeen and three-quarters ($17\frac{3}{4}$) drachms each. The contents of one package is to be dissolved in a pint of hot water, and it is best to have a bottle which will contain just one pint, as there is then no need for further measuring. The contents of one of the sugar packages is put into the bottle, and when filled with hot water the sugar soon dissolves, and it is ready for use. The dry sugar keeps indefinitely, but after it is once dissolved it sours if kept more than a day or two in warm weather; it is understood, therefore, that the sugar-water must be kept in a cool place, and if it should at any time become sour, as is easily discovered if it is smelled and tasted, it should be thrown out, and after the bottle has been carefully washed with boiling water, the contents of a fresh package dissolved. A milkman must be found who will serve every day fresh, good milk and cream. By good milk is meant ordinary milk, such as is easily procured in most cities, and not rich Jersey milk; and

in the same way the cream should be such as is ordinarily used in tea and coffee, and not the very rich cream of fancy cattle. The reason that ordinary milk and cream are recommended, is because they are within the reach of almost every one, and not because they are any better than the rich milk of high-bred stock. If Jersey milk was to be used, it would be necessary to analyze specimens, and then make the necessary calculations as to how to dilute it to obtain the desired relative proportions of the proximate principles. When the child is to be fed, the nurse should mix together two (2) tablespoonfuls of cream, one (1) of milk, two (2) of lime-water, and three (3) of the sugar-water, and then as soon as the mixture has been warmed it may be poured into the bottle and the food is ready for use. If the infant is healthy this quantity will not satisfy it after the first few weeks, and then double the quantity must be prepared for each feeding. Twice as many tablespoonfuls of each of the ingredients must be mixed together, making sixteen tablespoonfuls (about half a pint) in all.

This food should not be given any stronger until the child is eight or nine months old at least, but if the infant is a healthy one, it may take as much of it as it wants, but always of the same strength. A robust infant will often take three pints, or even more, in each twenty-four hours. It is an easy matter for any one to learn how to make the lime-water; and it is advisable to have it made at home, for a great deal is used, and if it is made at home much trouble and expense are saved.

With regard to the propriety of increasing, from week to week, the strength of any artificial food given to infants, there has been some question. Most authorities have advised that the foods should be increased in concentration until finally the infant is given pure cows' milk. The propriety of this procedure, during the earlier months of life at any rate, is very doubtful. Although there is some reason to believe that the quantity of solid ingredients in human milk increases from month to month as lactation goes on, such an opinion should be accepted only with great caution, for it seems likely that if there is any increase in the concentration of the milk after the colostrum has once disappeared, and the nursing process has settled down into its even course, the increase is so slight that it may be disregarded. Analyses show that the milk of a woman whose child is two months old does not differ

materially from that of one whose child is twelve or fifteen months old. If, then, nature has made no difference, which our means of analysis will detect, between the milk of a woman who has been nursing two months, and one who has nursed twelve, an artificial food which has been found to suit an infant of two months, should be made more concentrated only very gradually, and with careful observation of the effect upon the health of the infant. It is best, therefore, if the infant thrives and grows as it should, not to make any change in the food until after six to nine months of age have been attained.

With regard to the use of condensed milk as a food for young infants, I can only repeat what I said in my former paper, that I cannot believe that any article which has been canned, and kept for weeks or months, or perhaps still longer, can be so good as the same thing when fresh. My table of analyses shows the composition of the dilution of condensed milk commonly used in this city, and it shows that the proportion of fat is much too small, and for this reason, partly at least, it fails as a food. Its success is due to the fact that it contains nearly the same proportions of casein and sugar as exist in human milk. Dr. Ellwood Wilson is in the habit of directing that after the first few weeks a small proportion of fresh cream be added to the condensed milk, and this would render it still more nearly what is needed; this practice, which cannot be too much commended, if condensed milk is used, is not, however, at all a common one. Withal, I am unable to believe that condensed can be as good as fresh milk, if properly used. There are many other points of great interest, connected with this subject, which I should have liked to bring to your attention, but my paper has been already much too long.

DISCUSSION ON COMPOSITION OF MILK, AND INFANT FEEDING.

The President called attention to the following points for discussion :

1. Any method of analysis which is to be generally accepted must be susceptible of verification.
2. All previous analysts agree about the amounts of water, fat and ash; therefore, about the amounts of casein and sugar alone is there any disagreement.
3. Further, all agree about the sum of the amounts of these two elements (casein and sugar), but diverge widely about their individual amounts.

4. If it has been proved that, collectively, casein and sugar amount to about 8 per cent., and that the sugar is about 7 per cent., it then follows that the casein is only 1 per cent.

5. When subjected to the same method of analysis, human milk yields nearly 7 per cent. of crystalline sugar, and cows' milk about 4 per cent.

6. To be able to make a proper food for infants, we must first understand the composition of their natural food—human milk.

7. Is there any great and important difference between the casein of human milk and that of cows' milk, except that it is contained in the latter in a larger proportion?

8. In preparing cows' milk for infants, clinicians usually direct that it be diluted, and some sugar and lime-water added; but more precision of advice is needed.

9. What is the best method of making an artificial food like human milk?

10. What is the propriety of increasing the nutritive quality of the food from month to month, as the child grows older?

11. What can be said of condensed milk as a diet for infants?

DR. J. CHESTON MORRIS, in opening the discussion at the request of the Chair, said: It gives me great pleasure to pay my tribute of respect to the accuracy and industry which Dr. Meigs has so evidently exhibited in his investigations, though they have led him to very different results from my own. Accurate analysis, even of inorganic bodies, is difficult, as many of us know who have tried it; but accurate analysis of inorganic mixtures is far more so, from the changes continually going on in them.

A perfect method of milk analysis is probably not yet attained. Dr. Meigs having replied to a question from Dr. Morris that he obtained the amount of fat present by repeated washings of the milk with ether and alcohol, Dr. Morris resumed: The extraction of all the fat present is difficult, owing to the globules being surrounded by a thin membrane of casein; this has been proved by Mitscherlich, who advises the previous agitation of the milk with a small quantity of solution of caustic potash, when the ether immediately dissolves out the fat. Perhaps the best process yet devised is that of the Society of Public Analysts of Great Britain, given at length in various numbers of the *Analyst*.

The amount of water present in normal milk does not vary very much, nor does that of ash; but judging from the report of over 12,000 analyses, made by Dr. Viette for the Aylesbury Dairy Company of London, that of the fat in cows' milk does vary from 3 per cent. to between 5 and 6 per cent. Any result less than 3 per cent. is regarded as proof of adulteration. So far is the amount of water, ash and fat from being regarded as agreed upon among analysts, that the problem with them generally has been to obtain a method which would enable them readily to ascertain this latter factor; and Mr. Helner has constructed, as a means to detect adulteration, a table showing the relation between the specific gravity and the fat present, the solids *not* fat (i. e., casein and sugar), being taken as constants. To

what extent these are really so may be judged from the analyses of Dr. Viette, above quoted, showing a maximum of 10.31 per cent. and a minimum of 8.97 per cent. in over 12,000 analyses. The total average is about 9.5 per cent. in cows' milk. But all cows' milk is not alike; that from different breeds has marked peculiarities and differences, perceptible even to the eye and the taste. The milk from the Jersey cow is rich in fat, but poor in casein and sugar; that from the Durham is comparatively watery, and poor in fat and sugar, though relatively rich in casein; that of the Devon is nearly as rich in fat as that of the Jersey, and in casein as the Durham, and richer in sugar than either of them. The commercial uses of these breeds in butter- and cheese making bear out the above statements.

Another reason for thinking that 1 per cent. is too low an estimate of the amount of casein in human milk, is that such a proportion would hardly supply the needed amount of nitrogenous food to the infant. Lehmann tells us that the amount of milk secreted daily by the human female may be estimated at 2.2 per cent. of her weight; so that a woman of 150 pounds would produce 3.3 pounds milk, or 23,100 grains—rather more than a quart and a gill. If only 1 per cent. of this is casein, it would give only 231 grains; if she weighed only 120 pounds, there would be only 2.64 pounds milk, of which only 185 grains would be nitrogenous. Authorities on diet tell us that the human being requires food amounting to one-twenty-fourth to one-twentieth of his weight, of which one-fifth, or 1 per cent. of the whole, should be nitrogenous. Applying this rule to the infant, which has certainly larger needs for food for development, a weight of 10 pounds, or 70,000 grains, would require 700 grains of nitrogenous food—a figure nearly approximating that which would be yielded by a milk containing 3 per cent. of casein. In colostrum, however, we have a fluid poor in casein, but rich in sugar and fat.

Casein and sugar, we are told by Lehmann, are both soluble to some extent in alcohol. I have studied the question of the identity of casein of cows' and human milk, without being able fully to satisfy myself, but am inclined to regard them as differing mainly in coagulability. This may depend on the salts with which they are associated, as may also the acidity or alkalinity of the milk. These salts have, relatively to the serum of the blood, a preponderance of phosphates and potassium salts to the chlorides and sodium salts; and the acidity or alkalinity of cows' milk is influenced by their food. As a rule, I believe it is alkaline.

I am in the habit of directing, as food for new-born infants, a mixture resembling colostrum—one part cows' milk to two or three of water, with a little sugar, for infants of two to six months; equal parts of milk and water, warmed and sweetened, for infants over six months; two parts milk and one of water, etc. As to quantity and frequency, much must depend on the child.

DR. PARISH: I cannot pass judgment upon the chemical question, but the matter of substitutes for human milk is one that I am brought face to

face with every day in private practice, and in public institutions. I have had but little successful results with any of the prepared foods in the market, except condensed milk. I have frequently been called to see infants who have been seriously sickened by the use of such foods, or by other ill-advised articles of infant diet.

If I have care of a child from birth, who is to be brought up without human milk, I begin with condensed milk; during the first few days I direct that one heaped teaspoonful should be dissolved in thirty teaspoonfuls of water; about the end of the first week one teaspoonful of the milk in twenty-five of water; at the end of a month one teaspoonful in twenty of water; at about the end of the second month I have added one-fourth cream; at the eighth month a change is made to cows' milk, diluted with one-third water, with occasional use of beef-tea, etc., according to the age.

I do not think a child using condensed milk for sixteen or eighteen months continues to thrive so well as if the method stated is resorted to. Cows' milk is unsafe for young infants in the cities.

The addition of cream is advantageous by overcoming constipation accompanying the use of condensed milk, and by contributing very perceptibly to the nutrition of the infant.

If a child, dependent chiefly on cows' milk, is taken sick, with bronchitis or any febrile disease, or with any digestive derangement, I recur to condensed milk as a sick diet.

DR. MEIGS, in closing the discussion, said: I do not see that anything has been said to disprove what I have advanced, which was, substantially, that if analysis shows the water, fat, sugar, and salts of milk to equal a certain amount, then the casein can only equal the difference between that amount and the total quantity of milk taken. We know what these four substances are, and can identify and estimate them. What casein is, however, is not yet fully understood, but we cannot escape from the conclusion as to its small amount in human milk, unless the process of analysis be shown to be erroneous in regard to some one of the other four constituents. I have noticed, incidentally, that the clear ethereal solution of the fat of milk sometimes lets fall a precipitate after standing. This I at first thought to be casein, but as it melted when warmed, it must be some one of the forms of fat, but is insoluble in ether.

I do not think that the specific gravity is any guide as to the composition of milk. Mr. Wanklyn has shown its utter unreliability; nor do I think that we can distinguish the different amounts of sugar or casein in milk by the difference of taste or amount of coagulum. Milk-sugar has very little sweetish taste. The composition of milk is tolerably constant; it is not subject to such large variation within normal limits as urine is. Milk does vary much in its amount of fat. That first drawn from a woman who has not nursed her child for some hours, will usually contain only 2 or 3 per cent. of fat, while that taken just after nursing may contain 9 per cent.

As regards the amount secreted by women, very absurd statements—

much below the proper figure—have been made by some of the older authorities, but Dr. John F. Meigs succeeded in one case in getting as much as three pints in twenty-four hours.

The salt which exists in largest quantity in milk is sodium phosphate. Cows' milk is almost always acid, while human milk is almost always alkaline in reaction.

Most authorities agree that cows' milk must be diluted when used for infants. Why is this, if not because the casein is less in human milk?

I agree with Dr. Parish that most of the prepared foods are not good. Condensed milk is largely used by physicians in this city, and I believe its success is due to the fact that it gives only about 1 per cent. of casein as usually directed to be given. Condensed milk contains about 50 per cent. sugar. It is not unlikely that some of the milk-sugar is removed from it. Milk-sugar and casein are entirely insoluble in absolute alcohol, and alcohol will only take up very small quantities of milk-sugar as it becomes dilute, and only when it is very dilute will it take up any considerable amount.

My effort has been to place the demonstration of the amounts of the different constituents of human milk, so far as possible, upon a mathematical basis; and unless it can be shown that the estimate of some one of the four constituents—water, fat, sugar, or salts—is wrong, then it has been proved that human milk contains only the small amount of casein stated.

SOME NEW FACTS ABOUT ASTIGMATISM.

Read December 12, 1883.

BY M. LANDESBURG, M. D.

ASTIGMATISM is such an abstruse subject, that it should generally only be treated before the narrower circle of physicians who have made ophthalmology a special study, and if I beg leave to lay before you the results of my observations in regular astigmatism, there must be some special reason which induces me to make exception to the rule. The results of my observations open a new insight in to the nature of astigmatism; they mark a real progress in the knowledge of the latter, and furnish practical consequences, which may be utilized for the benefit of all those who are suffering from a similar trouble.

You know, gentlemen, that by astigmatism we understand that form of asymmetry of the cornea, in which the curvature of the latter is either different in the different segments of the same meridian, or in the different meridional planes. The first form is

called *irregular*, the latter *regular astigmatism*. Irregular astigmatism may be either acquired or congenital. It is very often associated with irregularities of curvature in the lens, and but very seldom admits of any remedial help or of correction by glasses.

It has been the scientific dogma of our days that regular astigmatism presents, in the greater majority of cases, a congenital and unchangeable optical defect of the cornea, which can only be neutralized by the selection of suitable glasses. The development of regular astigmatism, post partum, is considered to be of very rare occurrence, and to take place only in consequence of certain affections of the cornea, by spasm of the lids, and occasionally after iridectomy and extraction of cataract. In these instances, however, astigmatism is temporary only, and it generally subsides when the causal affection, which had produced the changes in the curvature of the cornea, has been removed.

My observations in regular astigmatism are in full contradiction to the prevalent opinion concerning the nature of this form of error of refraction. They have taught me that, certain conditions given, regular astigmatism may develop in any cornea; that it is apt to progress and to increase in degree, when the primary cause, of which astigmatism is only the effect—one of the many symptoms only of the morbid process, in which the eye actually is involved—continues to work. If you have such a case in hand and you correct the optical defect by cylindrical glasses, you only add a new injury to the existing ones; you only aggravate the morbid process; you consolidate a disorder, which is apt to be cured by appropriate treatment.

The conditions under which regular astigmatism may develop are: *Progressive myopia, with and without spasm of accommodation—spasm of accommodation in an emmetropic, myopic and hyperopic eye.*

In my first communication on this subject in von Graefe's Archives of Ophthalmology, xxvii, 2, I gave the history of fourteen cases which came under my treatment, either for progressive myopia with and without spasm of accommodation, or for the most various asthenopic troubles, based upon spasm of the ciliary muscle, in connection with myopia or hyperopia. All these cases were complicated with regular astigmatism. The degree of the latter varied from 1-36 to 1-10. An increase in the

degree of astigmatism was observed in two cases of progressive myopia, in connection with the progress of the latter. The treatment instituted for the asthenopic disorders, or for the progressive myopia, had the effect not only to cure the affection proper, but also to remove astigmatism entirely. The degree of astigmatism gradually subsided, keeping pace with the decrease of the other morbid symptoms.

I am now able to corroborate my first statements by additional facts. The latter are based upon a further observation of thirteen cases, in which the *transitory character of certain instances of regular astigmatism* has been fully established.

I shall not be guilty of wearying the audience by a monotonous exposition of all these cases, however interesting they may be. A brief summary of three cases will suffice to illustrate my proposition :

CASE 1. — The 12 year old boy McE. came under my treatment October 26, 1882, on account of weakness of his eyesight, which had rendered regular work impossible. He has been suffering for the last years from violent headaches, to which were lately associated noises in the ears. He had stammered before, but slightly and occasionally only. This disorder has now become permanent for the last few months. There were marked anæmia and nervousness ; the eyelids were in constant nictitation ; the external appearance of the eyes was normal ; all the other organs were in good condition.

Vision of the right eye was 12-70 ; concave 1-12 increased vision to 12-30 ; concave 1-12, combined with concave cylindrical 1-18, 65°, brought vision almost to 12-20. Vision of the left eye was 12-100 ; concave 1 20 increased vision to 12-40 ; concave 1-20, combined with concave cylindrical 1-26, 105°, brought vision to 12-20. There were besides weakness of the internal muscles and marked venous hyperæmia of the retina. The boy was not able to continue reading for a few moments, not even medium large print. He turned the book soon to the right, soon to the left side, raised and lowered it, and seemed to feel easiest by holding it in an oblique position to his visual line, the head turned on the vertical axis to the left. The eyelids, which were in constant nictitation, closed spasmodically on protracted efforts of accommodation, which had besides the effect to call forth lachrymation and congestion of the ocular conjunctiva, with a sensation of intense pain and pressure in the forehead and in the temples. Photophobia was not present, and the eye could stand even strong light with great ease.

I abstained from any internal medication, however tempting it was to try to build up the system by using tonics. My treatment consisted merely in absolute rest of the eyes and in the use of

duboisia, by which maximal mydriasis was kept up. With the abatement of the spasm of accommodation, which was marked by a decrease in the degree of myopia and astigmatism, and by an increase in vision, the general health improved, headaches and nervousness subsided, and stammering returned to its former condition.

The final result of the treatment, as noted down January 12, 1883, was as follows: Vision of the right eye 12-15; concave 1-42 increases vision to 12-12. Vision of the left eye 12-15; concave 1-60 increases vision to 12-10. No astigmatism in either eye. The weakest cylindrical glasses at my disposal, concave 1-70, impairs vision. Equilibrium of the muscles perfectly restored. Retina normal.

Examination repeated June 2, showed no change in the condition of the eyes; the latter have done their due amount of work without ever causing the slightest annoyance.

CASE 2.—The 10 year old boy B. came under my treatment October 2, 1878, for progressive myopia and asthenopic trouble, with the following condition of his eyes: Right eye, M. 1-16, V. 15-70; concave 1-16, combined with concave cylindrical 1 36, 95°, increases vision to 15-30. Left eye, M. 1-14, V. 15-40; concave 1-14, combined with concave cylindrical 1-24, 105°, increases vision to 15-20. The parents of the boy are highly myopic, and there is myopia in the respective families.

A three months' treatment by means of heurteloups and atropia had the effect to increase vision to 15-12 in each eye and to remove myopia and astigmatism. In spite of the many hurtful influences to which his eyes were subjected, the latter gave no cause of complaint until the fall of 1882, when symptoms of asthenopic troubles and of irritation developed in conjunction with the reappearance of myopia. Examination, made December 4, revealed: Right eye, vision 12-50, with concave 1-24, V. 12-30, combined with concave cylindrical 1-36, 90°, V. 12-20. Left eye, V. 12-100, with concave 1-10 respectively 1 9, V. 12-50, combined with concave cylindrical 1-30, 90°, V. 12-20.

A two weeks' use of duboisia and perfect rest of the eyes improved somewhat the condition, but treatment proper had to be deferred until the summer vacation. June 25, 1883, examination showed: Vision of the right eye 12-50, concave 1-18, increases vision to 12-30; concave 1-18, combined with concave cyl. 1-20, 125°, increases vision to almost 12-15. Vision of the left eye

12-200, concave 1-8, increases vision to 12-50; concave 1-8, combined with concave cyl. 1-18, 105° , increases vision to 12-20. Weakness of the internal muscles, marked retinal hyperæmia. Region around the macula lutea slightly suffused.

The use of duboisia and heurteloups, and perfect rest of the eyes, gradually led to perfect recovery. The final result, noted October 3, was: Right eye, V. 12-12; it bears concave 1-70, but not concave 1-60. Left eye, V. 12-15, concave 1-45, resp. 1-36, increases vision to 12-12. With both eyes (without the use of concave glasses), vision is 12-10. Astigmatism entirely vanished. Equilibrium of muscles restored. Retina normal.

CASE 3.—The 14 year old boy W. was brought to me December 27, 1882, on account of asthenopic troubles and weakness of his eyes. Examination showed: Vision of the right eye, 12 50; convex 1-70 up to 1-42, increases vision to 12-20. Vision of left eye 12-40; convex 1 60 up to 1-42, increases vision to 12-20. Cylindrical glasses do not improve vision. There is marked spasm of accommodation.

My proposed course of treatment was not agreed to at the time. The boy continued working until March, 1883, when his eyes failed totally. He resorted to another oculist, who ordered him the following glasses for near work: *Concave 1-40, combined with concave cyl. 1-36, 120° , and prism 2° , base inward for each eye.* They acted at first like a charm, and patient was able to continue his studies for a few weeks, but then the condition changed to the worse. A modification in the glasses—*concave 1-30, combined with concave cyl. 1-36, 120° and prism 3° , base inward for each eye*—ordered by the same physician, proved without avail. The slightest effort of accommodation provoked the most agonizing headaches, nausea, and even vomiting; vision became as bad for distant as for near objects; the general health suffered considerably. Patient lost flesh and appetite and became of very irritable temper. In this condition he was entrusted to my care, June 28, after a treatment for dyspepsia had proved a total failure.

The examination showed: Vision of the right eye 12-70; concave 1-20 increases vision to 12-40; concave 1-20, combined with concave cylindrical 1-20, 105° , gives vision 12-20. Vision of the left eye 12-100; concave 1-18, resp. 1-16 increases vision to 12-50; concave 1-18, combined with concave cyl. 1-30, 110° , gives vision 12-20. Medium small print (Jaeger 3) is read with great effort,

the book being kept close to the eyes and askant to the visual line, the head turned on the vertical axis somewhat to the left. The lids are in constant nictitation with intervening spasmodical contractions. There is marked lachrymation and photophobia. Both retinæ show intense venous congestion.

The same treatment as has been described in the second case, gave, October 2, the following result: Right eye, vision 12-10; left eye, vision almost 12-8. Both eyes bear convex 1-60, but not a higher number. With both eyes (without the use of glasses), vision is 12 8. With the help of convex 1-60, vision reaches almost 12-7. No trace of astigmatism. All disorders vanished. General health good.

DISCUSSION ON ASTIGMATISM.

DR. SHAKESPEARE: My experience has led me to conclusions, in some respects, similar to those advanced by Dr. Landesberg. The prevalent belief that regular astigmatism is congenital, constant in degree, and correctable by the same cylinder through life, is inconsistent with facts which I have frequently and for a long time observed. I have found that regular astigmatism is by no means always congenital or stationary, and have reached the conviction, both by clinical experience and experiment, that in very many cases regular astigmatism not only is acquired, but also often is variable in character and degree. But when Dr. Landesberg affirms that regular astigmatism is never congenital, I disagree with him, for I am sure that asymmetry of the curves of the eyeball affecting its optical qualities may begin *in utero*. In particular, asymmetrical curves of the back portion of the eyeball are sometimes recognizable at birth. Dr. Landesberg confines his observations to cases of progressive myopia, and to simple myopia; and he explains the production of acquired astigmatism in his cases by the irregular actions of the muscles in the *interior* of the eyeball. I go further and affirm the existence of acquired regular astigmatism, and its variability in degree and character, in cases of hypermetropia as well. Moreover, I am convinced that there are forms of acquired regular astigmatism, even of a more or less lasting type, wherein the *external* muscles of the eye are chargeable with the asymmetry of one or more of the optical curves. Several years ago I had occasion to demonstrate that the dimness of vision produced by pressure with the finger upon the eyeball, is not the result, as is believed and taught, of compression of the nerve-fibres of the optic nerve and retina, and the consequent loss of conductivity of luminous impressions; but, on the contrary, is due almost entirely to an artificial regular astigmatism of the cornea, and perhaps also of the fundus temporarily caused by the pressure. The proof is simple and convincing. My acuity of vision is above normal. When I place a concave cylinder of

eight inches focus before my eye, my vision is at once blurred so that I can scarcely distinguish the large E of Snellen's test-types at fifteen feet.

If I hold the axis of the cylinder vertical and gently press with my two fingers upon the eyeball in the horizontal meridian, so as to increase the curve of the cornea, the acuteness of vision immediately jumps from $\frac{15}{200}$

to $\frac{15}{60}$. If the axis of the cylinder be horizontal and I apply the pressure to the vertical meridian, the acuteness of vision is at once raised from $\frac{15}{200}$ to $\frac{15}{40}$, and this, too, while the eyelids are held wide open. This last remark tends to explain, in a different manner from that usually advanced, why many astigmatics are so prone to squint the eyelids in endeavoring to see distinctly.

Spasmodic or habitual contraction of the orbicularis, when associated with effort on the part of the levator to keep the lids open, is thus capable either of producing an astigmatism, or of correcting it.

So, also, derangement of the pressure upon the eyeball exercised by the recti-muscles can produce or modify astigmatism.

These views I, for several years, endeavored to impress upon the attention of classes at the University of Pennsylvania.

DR. RISLEY: For many years I have been thoroughly convinced that the cornea was subject to temporary changes of curvature. My attention was first directed to this possibility several years ago, by the occurrence of a large sty on the left upper eyelid of a young lady, who was at the time under treatment for an existing error of refraction. The eyes being under the influence of a mydriatic, she selected for each eye $+ \frac{1}{48}$ cyl. ax. 90°.

The mydriatic was continued for some reason, and at her second visit the inflammation in the left eyelid was present. With the right eye she selected the same glass as before, but with the left she now selected $+ \frac{1}{24}$,

an increase of one-half in the degree of astigmatism. The sty having disappeared, she once more selected the glass at first chosen.

My case books are replete with cases illustrative of the point brought out by Dr. Landesberg. Indeed, a comparison of the manifest error of refraction with the glasses selected after paralysis of the accommodation, will, in a very large group of cases with well-marked asthenopic and retino-choroidal irritation, exhibit precisely the change set forth in the cases presented to-night.

These changes must certainly be known to all ophthalmic surgeons. Not only does the astigmatism frequently disappear wholly or in part, but the apparently myopic refraction gives place to hypermetropic. I have many times taken concave glasses from patients who needed convex ones to correct the existing error. This mistake is especially liable to occur where correcting glasses are ordered without the use of a mydriatic. The change which has most frequently fallen under my notice, is in the direction of the

meridian of highest curvature; *e. g.*, many patients will, before mydriasis, select a concave cylinder with its axis horizontal; but after the paralysis of accommodation a convex cylinder with its axis vertical, *i. e.*, at a right-angle to the former direction, will prove to be the proper correcting glass. The change in the form of the eyeball is by no means confined to myopic eyes, or to existing spasm of accommodation, but I am convinced is often due to the prolonged pressure of the orbicularis, as in cases suffering from undue sensitiveness to light. I recall to mind the case of a physician who came, having practically lost the sight of the left eye from *retinitis mac. lutea*. The right was now troubling him very much, and he was in great dread lest this also should be lost. He complained bitterly of the painful glare of light from the roads over which he was constantly driving in attending to the daily routine of his professional duties. The examination revealed much retinal irritation and hypermetropic astigmatism. After prolonged use of a mydriatic, he selected

$$+ \frac{1}{30} \text{ s } \ominus + \frac{1}{30} \text{ cyl. ax. } 90^\circ \text{ V } \frac{20}{20}.$$

This glass was worn with entire comfort from April, 1880, until August or September, 1881. He then began to realize that his glass was an annoyance to him, and the following November returned for advice about it. His vision was now very much diminished by his glass, although nearly

perfect without it. A mydriatic was again instilled, and with $+\frac{1}{16} \text{ sp. V } \frac{20}{20}$.

I am of the opinion that in this case the pressure of the lids upon the ball, caused by his endeavor to avoid the light, had been sufficient to change temporarily the curvature of the cornea; that under treatment the dread of light disappeared, and with it the pressure of the orbicularis; This removed, the ball gradually resumed its original hypermetropic form. Another cause of temporary change in the refraction of an eye, not mentioned by the lecturer, is conical cornea, or at least changes due to distension of the cornea, not always conical in form, but of such a character as to cause high grades of astigmatism susceptible of partial correction.

The case of Miss B. will illustrate this condition. She first consulted me in 1877, for weak eyes, failing vision and violent headaches, which were attended with nausea and a long catalogue of nervous symptoms.

$$\text{O. D. V} = \frac{20}{200}, \text{ O. S. } \frac{20}{30}.$$

After the prolonged use of atropia, and the most careful measurement, I ordered for

$$\text{O. D. } + \frac{1}{24} \text{ c ax. } 165^\circ \text{ V } \frac{20}{30}, \text{ O. S. } + \frac{1}{60} \text{ s } \ominus + \frac{1}{60} \text{ c ax. } 15^\circ \text{ V } \frac{20}{20}.$$

These glasses, for some time, proved of great benefit, but three months later she returned, complaining of the former symptoms and dissatisfied with her glasses. Atropia was again instilled, this time for two weeks,

and the refraction error once more corrected. O. S. remained unchanged, but the correcting glass for O. D. was now

$$+ \frac{1}{36}^{\circ} \text{ ax. } 165^{\circ}; \ominus - \frac{1}{60}^{\circ} \text{ ax. } 75^{\circ} \text{ V } \frac{20}{30}$$

Two years later, Miss B. called simply to say that she was well. Eye symptoms and headache had entirely disappeared. In May, 1882, she returned with a renewal of all her former symptoms.

$$\text{O. S. V } \frac{20}{30}, \text{ O. D. } \frac{20}{200}?$$

Ophthalmoscopic examination showed the characteristic appearances of conical cornea. She was now treated with instillation of eserine sulphate and the application of a pressure-bandage, until the following October, with occasional intermissions. During this time, numerous trials failed to secure any improvement of her vision by glasses. The pressure was then omitted, but the eserine continued. Her pain had subsided, but would come on again after least endeavor to use her eyes for near work. In December, 1882, an attempt was once more made to correct the refraction of the right eye. It was found that

$$+ 1. \text{ D. sph. } \ominus - 5. \text{ D. cyl. ax. } 75^{\circ} \text{ gave V } = \frac{20}{L}.$$

This glass has been worn constantly since, and she is now able to use the eyes moderately without pain.

WHAT IS MEANT BY NERVOUS PROSTRATION?

Read December 19, 1883.

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THE popular conception of the condition now known as "nervous prostration" is a state of debility, in which nervous derangements predominate. A man actively engaged in business or in public life, presently finds himself unequal to his daily tasks; he suffers odd sensations in his head; his digestion is disordered; he is weak; wakefulness, mental depression, and a thousand and one new sensations of strange character and fearful portent, are superadded. The unfortunate subject of these ills now recoils from his work, gives himself up to the consideration of his symptoms, and relaxes his hold on the interests and occupations of his life. All the world declares that he has "nervous prostration," and this explanation satisfies. Physicians say "neurasthenia"

or "hypochondria," according to their habits of mind or to their training. Sometimes this condition is called the "American Disease." Indeed, there is a general notion, widely prevalent, that neurasthenia is a peculiarly American malady. The late Dr. Beard was the apostle of this dispensation, and he not only was noisy and persistent in his advocacy of that view, but claimed, indeed, to have first clearly defined neurasthenia, and to have classified under this designation the numerous symptoms pertaining thereto. If we cannot admit Dr. Beard's claim in its entirety, if we experience repulsion at his tremendous but unconscious egotism, we are still compelled to acknowledge that his work in this connection is the most important that has appeared. He was peculiarly fitted to differentiate this malady by reason of the quickness and acuteness of his intellect, his power of analysis in its subtlest aspects, and his far-reaching, his omnivorous faculty for related facts.

The term *neurasthenia*, advocated by Beard, is by no means of recent origin. The corresponding French word, used in the same sense as we now employ it, has been a stock word of French neurological medicine for fifty years. Under the terms spinal irritation, hysteria, hypochondriasis, the nervous state, etc., symptoms of the same character as those now included in the word *neurasthenia* have been described. Besides the general state, similar derangements of functions of particular organs have been separately considered, as palpitation of the heart, headache, flatulence, impotence, etc. In the word *neurasthenia*—popularly, nervous prostration—the whole morbid complexus is included. The question I have to consider is whether this is a real, a substantive disorder. Are the notions now generally entertained about it founded on a true conception of the condition?

I need not enlarge on the importance of a correct understanding of a morbid state, which is supposed to be due to the conditions of modern, especially of American, life. Without stopping now to question the soundness of the prevailing doctrine, I will place before you the clinical history of two cases, representatives of the two types of *neurasthenia*. These may be designated respectively as the *congestive* and the *anæmic* varieties. The latter are greatly more numerous, but the former are not uncommon, as Beard admits.

CASE I.—THE CONGESTIVE TYPE.

Mr. —, æt. 44, president of one of the largest railroad corporations of the West. He is now a robust man, 5 feet 10 inches in height, 196 pounds in weight, and has a very dark complexion, his type of constitution being the so called bilio-nervo-sanguineous. Beginning his career at an early age in a subordinate position, he has, by the force of a superior intellect and of a physique that no labor could subdue, risen to the highest office, and now controls vast interests. Ambitious, enterprising, resolute, he has carried these faculties into all his work, and has shrunk from no tasks, however severe—from no responsibility, however onerous. As he has risen in position, social engagements have also added to his burdens. His mode of life has changed to some extent. His habits have become more sedentary, although diversified by frequent railroad journeys; the pleasures of the table, including wine-drinking and late suppers, have been more and more indulged in; excessive smoking has been added to these indulgences; and thus, whilst his physical powers have been slowly impaired by bad hygiene, the demands on his mental powers have increased. Extensive interests, uncertain, often precarious, business arrangements, and the incessant watchfulness required when vast combinations may be wrecked through failure at any point, demand the highest use of every faculty; and thus to work is added worry.

Three years ago Mr. — observed that he was not feeling well, and that he could not work as before. He became dull, especially after meals, had a constant headache, dizziness and throbbing of the temples; he applied his mind with difficulty, and all of the head symptoms were increased by the efforts made; he had a good, rather a keen, appetite; a heavily coated tongue, flatulence, constipation and some colic pains. The bladder was rather irritable, especially at night; sexual inclination had declined with lessened power, and various ill-defined but annoying sensations were felt about the penis scrotum and perineum. During the first year the symptoms increased; the attacks of vertigo were sometimes very severe, so that he had to support himself for a moment to save him from falling. On several occasions he became very much dazed, even lost consciousness momentarily, and once wandered some distance from the proper route he was taking. Anomalous sensations of creeping and crawling, coldness and tingling, and often a burning heat, were felt in the scalp; sudden detonation in the centre of the head apparently; buzzing and singing in the ears, and very constant headache, were also experienced. In the extremities, the tongue, and the genitals, there were felt peculiar tingling, numbness, coldness, creeping, and similar sensations. During the whole time of the existence of his symptoms, Mr. — suffered from depression of spirits, a deep melancholy in fact, and he lived in constant apprehension of failure of mind.

Physicians whom he consulted in the West, located the malady in the brain, diagnosed cerebral hyperæmia, the prelude to softening.

When Mr. — came to see me, sixteen months ago, the symptoms just

detailed continued, and were rather increased than diminished. The objective examination furnished the following details :

His face is full, the eyelids puffy, and the lower lid swollen into a bag; the conjunctivæ are injected, the sclerotic muddy, and the pupil sluggish in movement. On ophthalmoscopic examination, the fundus is seen to be injected, small vessels prominent, veins swollen. There is no optical defect, except that due to his age. The membrana tympani is also rather deeply red, and vessels too small to be seen under ordinary circumstances are now in view. Hearing is unaffected. Motility, sensibility—the tactile, pain and temperature senses—are unaffected ; and the reflexes remain normal, although probably a little sluggish. The electrical reactions are normal.

His tongue is heavily coated, the breath foul. His appetite is good, but a sense of fullness at the epigastrium persists for several hours after meals ; acidity and eructations of rather foul gas now and then occur. The stools have the normal appearance—consistence, color and odor. The urine is copious, acid, specific gravity rather high (1025 to 1030), and there are traces of sugar, as is usual under such circumstances.

The action of the heart is good, the pulse regular, the tension of the vessel rather high. The respiratory movements and murmurs are normal. The area of hepatic dulness is rather enlarged, and the splenic dulness seems, also, to be increased.

Subjectively the following symptoms are experienced : Various strange sensations in the scalp ; a persistent headache ; blurred vision at times ; vertiginous feelings occurring irregularly and of varying severity ; despondency, vague apprehensions ; fear of places, especially of crowded assemblages ; difficulty of deciding questions very trivial or otherwise, in place of former promptness ; impaired memory, for persons, names and things.

Notwithstanding this extended list of symptoms, Mr. — did not have an ill look, but, on the contrary, on superficial examination, appeared to be robust. To him and to his immediate family the situation seemed in a high degree alarming. The surrender of his position and his business interests was regarded as imminent. To the apprehension awakened by his head symptoms was added the diagnosis of cerebral congestion, and hence the profound melancholy into which he was plunged.

Commentary.—My conclusion was that the disturbance in the functions of the brain and nervous system were secondary to derangement of the assimilative processes—of the primary and secondary assimilation—and that to the functional disorder thus caused are added the effects of introspection, and the realization by the centres of conscious impressions to an unusual extent, of ordinary peripheral excitations. My reasons for coming to this conclusion will appear hereafter. The remedies consisted in a careful regulation of the diet, in baths, exercise, in a reduction of the hours devoted to work, but not the cessation of work ; in

the use of a laxative quantity of sodium phosphate daily, and in the administration of the aqueous extract of ergot, with the chloride of gold and sodium, and a minute quantity of bichloride of mercury. If time and space would allow, the details of the hygienic management—so important in these cases—could very profitably, I think, occupy our attention. But I must pass on to the next case.

CASE 2.—THE ANÆMIC TYPE.

Mr. ———, æt. 57; lawyer by profession. His type of somatic constitution is the nervo-sanguine; weight, 145; height, 5 feet 9 inches. He has immense mental energy, extraordinary quickness of perception, a capital logical and critical faculty, and fine oratorical power. These native abilities, conjoined with extensive cultivation, soon placed him amongst the foremost men at the bar of the city where he practiced, and have long maintained him in that position. For many years he has been a dyspeptic, and suffered much from eructations of gas, from acidity and flatulence. At times—months, even years intervening—he has experienced very severe seizures, accompanied by extreme mental depression, alternating with as extreme mental exaltation. During the past five years he has had two attacks of gout, neither severe nor protracted. During the whole course of his professional life he has sustained no reverses, encountered no other anxieties than those of a successful lawyer, and has been rather singularly free, indeed, from the worries of life. Receiving last summer the nomination as a candidate to an important office, this cultivated gentleman, scholar and lawyer, this man of nice tastes and high tone, entered on a canvass marked by vituperation and slander to an unusual extent. About the same time some business interests became entangled and caused no little worry. During the campaign he visited some malarious districts and spoke several times at night in the open air. A speaker of great readiness and power, he never suffered from any considerable fatigue after public speaking, and hence he was now surprised to find himself exceedingly tired after even a brief effort. He began to have drenching night sweats, lost his appetite, grew weak and was compelled to return home. It was then ascertained that he had malarial fever, and was treated accordingly. But at this time, and subsequently, symptoms not necessarily of malarial origin appeared. He became frightfully dyspeptic, had enormous eructations of gas, and very considerable flatulence; his arms and legs had a numb feeling, attended with “pins and needles;” he walked with some difficulty, partly because of weakness; he was somnolent and slept a good deal, and his spirits were extremely depressed, especially on awaking in the morning. During these periods of depression he was so overwhelmed with despondency that he was apprehensive he would lose his self-control entirely.

When he placed himself under my charge, he had still a slight daily paroxysm of fever, the exacerbation occurring in the morning, but this disappeared in a few days under the action of some efficient doses of quinine.

He was very weak, pallid, and emaciated, and slept a good deal of the time. He had no headache; his vision was rather dull, and ideas and speech slow. Every morning on awaking he was profoundly melancholic, and all the annoyances which the campaign had developed were gone over in his mind. He could talk of nothing else, think of nothing else than his ill feelings and the disagreeable political and personal slanders of which he had been made the victim. He complained much of the numbness of his hands, of weakness in the limbs: and he talked incessantly of his depressed feelings. The bladder became irritable, and he was compelled to rise every two or three hours during the night, the urine being acid, and depositing heavily of uric acid. Presently the somnolence was displaced by insomnia, and he slept less and less, and rose in the morning haggard, exhausted and horribly nervous and depressed. Ordinary hypnotics proved unequal to the effort to force sleep, and increasing doses of chloral became necessary. His mental activity, heretofore so remarkable, declined, and the effort to force his mind to the performance of any work, such as letter-writing, caused a sensation of fatigue. He also became undecided, even in small matters, ceased to have any inclination to go out and mingle with the public, and grew more and more averse to political movements. He reached a point finally when to meet strangers caused him great distress, excited the circulation, and induced a cold sweat.

As it became indispensable that he should resume the canvass, he made a strong effort, and notwithstanding the fatigue, mental and moral depression and exposure of public speaking, handshaking, and other matters of political expediency, he actually improved somewhat. The insomnia, irritable bladder, and hypochondriasis, however, continued, but to a less degree. In a few weeks, by means chiefly hygienical, I succeeded in stopping the chloral, natural sleep was resumed, although it remained somewhat fitful. Suitable dietetic regulations, baths, exercise and medicines, *pro re rata*, removed, or at least greatly modified, the principal symptoms. Two weeks at Atlantic City accomplished no little good, and when he returned to Philadelphia last week he appeared to be nearly his old self.

Commentary: In this case we have exhibited that complexus of symptoms entitled neurasthenia or nervous prostration in its anæmic form, produced by several factors—moral and somatic. The moral were very influential, but unless the conditions producing bodily depression had occurred, the former causes could hardly have effected such results. Long-standing dyspepsia had prepared the way; malarial intoxication and fatigue contributed an important series of changes, and upon this weakened bodily state were precipitated crushing moral influences.

These cases, whose histories I have just read, are typical—each is the representative of a group. The causes are complex; the effects are not limited to one organ, or set of organs, but involve

the system in general. To name this malady from the disturbance in one system seems to me an error, unless the definition is sufficiently elastic to include all the functions affected. Neurasthenia names one, only, of the parts involved. To entitle this the "American Disease," is a strange misnomer. It might with more propriety be called the "French Disease," for a condition known as "the nervous state," as "nervism," as "neurasthenia" and similar terms, has been recognized and frequently described by French writers from an early period in this century. In France have existed the causes in the most influential form. The frequent political convulsions, the exacting social life of the great cities, and the harassing struggle for existence inseparable from the state of the great mass of the population, induce—if any mere external conditions can—that which is called nervous exhaustion. There are two factors supposed to be especially influential in this country—work, and our exciting political and social life. I believe that the effect of these is greatly overrated.

The brain, of all the organs of the body, illustrates, in the most perfect manner, that which has been happily styled "the principle of least action." That is, to execute given tasks, it expends the least possible force, or, to express the same idea in another form, its work is done with ease, with the minimum of effort. Given a certain amount of repose—sleep—and supplied with proper nutriment—healthy blood—the brain will do its allotted work continuously during its working—the waking—hours. So far from being injured by severe labor carried on under normal conditions, the brain is improved by it. Mental activity, like muscular exercise, keeps the brain in a healthy state. When, therefore, a man says he is suffering from the effects of mental overwork, I want to know what his vices are. Worry may be one of these. Worry is exhausting. The worries of life do infinitely more harm than the work of life, how onerous, soever, it may be. The cases I have just read illustrate this.

• I deny that life is more exciting on this side of the Atlantic. The one prize of life is money, and to get possession of it is the supreme purpose, to the attainment of which every energy is put forth. Is it less so elsewhere? Who are the peoples that despise money, and make no effort to obtain it? Here life is less exciting, because our political condition is stable, and but comparatively little exertion is required to secure a comfortable subsistence. I

am speaking now of the mass of the population, and not of the few consumed by ambition for political and social distinction, or led by a pitiless greed. It is the very ease and luxury of our American life that cause mischief. It is the indulgence in eating and drinking, the abuse of alcohol and tobacco, sexual excesses, sedentary habits, and too luxurious lives generally, that induce the state of the system called nervous exhaustion. If I had time, each of these should be considered, in relation to this subject. In the first case I narrated, the pleasures of the table and disordered assimilative functions caused the trouble. In the second case, dyspepsia, malarial toxæmia and unusual fatigue were the pathogenic factors. In both, the effects of these causes were increased by moral influences—in one, the anxieties involved in vast business enterprises; in the other, the excitement of a hot political contest. These moral causes would have had no injurious effect, had not the somatic conditions been unfavorable.

I come now to the most difficult part of my subject. I have to answer this important question: Why are the somatic derangements caused by the conditions referred to, in some cases accompanied by the mental and nervous symptoms which belong to neurasthenia? Why do some subjects with indigestion and assimilative disorders, or with the results of dyspepsia and malaria, suffer from the derangements of the mental and nervous functions, and not others? I might here take refuge behind an accepted generalization, and say that the presence or absence of the neurotic type of constitution explained the difference in the result. There is aptness in this explanation, but it is not entirely adequate. There is a mental condition of great importance, and unless we comprehend this, we fail to realize all the possibilities of the nervous side of these cases. I, however, barely hint at the main points, under these circumstances. Besides, I wish to avoid a too metaphysical discussion of the subject.

In the conduct of life every man who has a position to make or to maintain, exerts a certain moral force to hold himself up to his work. Some men are so happily constituted that they are quite unconscious of the effort and stand in the front, serenely confident. Others are all the time laboring; they feel it and know it, and like the soldiers of Thomas's corps at the battle of Chickamauga, sorely pressed, now and then looked back, to see whether their grim and resolute commander was still behind them

with his invincible courage. Men conscious of the effort making to keep up, need but little excuse to surrender themselves to their sensations. At the present time nervous prostration is much feared; its symptomatology is a common subject of discussion; and hence, familiar with its character, a man who is arrested in his career by some of the ailments supposed to belong to it, his imagination readily supplies the rest. When a man begins the study of his bodily sensations, having a certain model in his mind, he has little difficulty in filling out the details. All the world knows that when the attention is strongly fixed on an organ of the body, functional disturbances of it ensue, and finally structural changes may be induced. No part of the body is without sensation, even in health. To perceive these sensations the attention needs to be withdrawn from external things, and concentrated on the part. Thus it is when the subject of neurasthenia pursues his introspection, he becomes conscious of numerous sensations, which, because now felt for the first time, are new. Under these circumstances, also, the seat of conscious impressions becomes more acutely perceptive. Suggestion adds its quota of symptoms.

To the indefinite and multiplying nervous symptoms developing thus subjectively, must be added the reflex. Headache, vertigo, *tinnitus aurium*, amaurosis, diplopia, hallucinations and illusions, defects of speech, paralysis, are reflex symptoms on the part of the brain; palpitation, intermittent pulse, angina pectoris, laryngismus stridulus, asthma, are amongst the reflexes of the respiratory organs and heart; neuralgia, anæsthesia and other disorders of the sensory nerves, and local paralyses, affections of the motor nerves, included amongst the nerve reflexes, may all be dependent on reflex excitations proceeding from the stomach. Indeed, there is no symptom in Beard's catalogue of those belonging to neurasthenia that may not be due to merely reflex influences having their initial seat in the digestive apparatus. It follows that the term neurasthenia, or its common equivalent, nervous prostration, is either inadequate, or it expresses too much. Inadequate if the complex of symptoms includes the functional disturbances of all the organs affected, expresses too much if the malady is a merely nervous one.

In reply to the question: "What is meant by nervous prostration?" I respond, "a disease usually functional, situated in

one or more organs, during the course of which reflex disturbances of the brain occur, and numerous subjective sensations in all parts of the body are realized by the consciousness."

I deny that neurasthenia is a primary nervous affection, or that it is a substantive disease. I hold that it is symptomatic and secondary.

This conception fixed in the mind, the treatment of neurasthenia is successful or unsuccessful according to the measure of our skill in localizing the initial disturbance, and in addressing our remedies to that as well as to the general state.

DISCUSSION ON NERVOUS PROSTRATION.

DR. MILLS, in opening the discussion, by request of the Chair, said: I understand that Dr. Bartholow denies that a disease exists, primarily nervous in origin, which can be called neurasthenia. He classes all cases so called under two heads, congestive and anæmic, and holds that the symptoms presented are chiefly reflex effects of digestive or other visceral troubles. I have no doubt that many cases are to be thus explained, but we have others in which the cerebral condition is primary. In cases with the symptoms as detailed in the paper, we may perhaps clearly ascribe them to the causes and conditions referred to by Dr. Bartholow, but other cases may be explained in a different manner.

In individuals whose higher ganglionic centres are so constituted, from bad inheritance or poor training, or both, that they cannot bear much strain, when subjected to this strain, these centres exhaust. We have certain functions called organic functions, respiration, vaso-motor action, etc., with centres in medulla oblongata and spinal cord. These functions must be maintained as long as the individual exists. Their centres must be nourished and sustained in a uniform way. Presiding over automatic movements, they must be kept in the highest tone, must have good blood and plenty of it. It is a principle brought out in the paper that local diversion of blood to any one organ or part will take it from other organs or parts; and we may, in accordance with this, have the higher brain diverting blood from the lower, or lower brain from the higher. We have *nervous* symptoms, the result of this overstrain of nerve-centres, and the disturbance of the equilibrium of the circulation, thereby brought about.

I am inclined to differ from Dr. Bartholow, and agree with Dr. Beard, as to the propriety of the term "American disease." The social and business exigencies in this country are different, are more taxing than in Europe. In England, for instance, men become mature and enter public life at later periods than here. In France the difference is not so great, but the more absolute division of society into grades and castes prevents too fierce a struggle for high position. Here every man has the chance to rise to the

highest position, and men enter especially political life in youth. Americans are not trained for special lines of life-work. They often attempt work too high for their mental powers and break down.

DR. TYSON: I have many times asked myself the question, "What is nervous prostration?" The answer was always, "It is not a nervous disease, certainly not an organic nervous disease, and probably not a functional one. It is rather a condition of muscular fatigue, and it may be nervous fatigue. Most of the cases are accompanied by digestive derangements, which are responsible for many of the symptoms, most of which are reflex." At the same time, I admit that some of the symptoms are puzzling, and not easily explained. Among these is the pain in the back of the head, often referred to as the result of "nerve tire," and sometimes regarded as an important indication of cerebral disorder, which it is not. In women, uterine derangement is often responsible for the complex symptoms known as nervous prostration.

The point, however, that attracted me in the paper read was allusion to the condition of the urine in one of the cases. It was described as dark brown, of high specific gravity, containing a trace of sugar. I have seen such cases, and think it not unlikely that the apparent sugar reaction is really due to uric acid, which is often abundantly present in these urines of deranged digestion. If we get rid of the excess of uric acid by allowing it to precipitate spontaneously, or precipitate it by the addition of an acid, the sugar reaction with the copper tests does not take place. Other characters of the urine, particularly its dark color and scantiness, are not usual to sugar-containing urine.

DR. ESKRIDGE: The most important point in the subject is whether the condition called neurasthenia is primarily a nervous disorder, or a disturbance in other parts of the body. I would have been better pleased if Dr. Mills had elaborated more thoroughly his statements as to the organic functions. He said these functions must be well-nourished, and that failure in them is due to failure in the upper nervous system. I would have liked if he had gone further in his explanation. Is this failure due to disturbance in the brain and upper portion of spinal cord primarily, or to failure in other parts of the body. Those who have paid attention to this subject say that breakdown does not occur unconnected with either worry or vice. I have never seen a case from mental over-work alone. In five cases of neurasthenia of which I have notes, one was from sexual excess; two from worry connected with family troubles; another from alcoholic excess, with sexual vices; and the fifth from sunstroke, followed by over-sexual indulgences. In the last case it seemed as if the brain was primarily at fault. It appears to me that nervous prostration, so-called, is a depressed condition of the whole system, the trouble manifesting itself as a general nervous condition secondarily, after organs other than the brain have been primarily affected. The therapeutics also favor this view, because the cases are benefited, not by agents addressed to the nervous system alone, but by hygienic and tonic measures.

DR. GLASGOW: I have come to regard these cases as not nervous in origin, but as largely due to digestive troubles. They are cured by atten-

tion to the disordered functions. I have known a short residence in Atlantic City, without any medical treatment, to suffice for a cure.

DR. BARTHOLOW, in closing the discussion, said : The great question is, Is neurasthenia functional, or does it arise from organic change in the nervous system? I maintain that it is largely reflex and frequently from gastric disturbance. Neurasthenia in women is associated with an anæmic condition and ovarian and uterine disorders. They are pale and weak, and if they are subjected to any moral or mental trouble give way, but if they are well nourished sustain any kind of shocks without suffering in health.

In regard to the term "American disease," and to the claim that the peculiar conditions of American life are causative of neurasthenia, it will suffice to say that in some of the European states a higher grade of education is maintained than in this country. In Prussia, for instance, every one is taught to read; education is more general than in Massachusetts. The struggle to maintain existence, and hence the demands on the brain, are severer than here, and although neurasthenia is said to be uncommon, there are diseases similar to that called nervous exhaustion in this country, arising from similar causes.

Worry has been mentioned as a cause of nervous exhaustion; now, worry hurts a man just in proportion to his condition. If he is in good health, or phlegmatic in temperament, the worry may be well borne, but if he is out of health, worry will have a powerful effect on the nervous system. I do not deny that various causes may produce brain disease, but I deny that the so-called neurasthenia is due to an organic lesion of the nervous system. I maintain that it is part of a morbid complexus; a reflex condition, in large part, of maladies situated in the stomach, the liver, the uterus or other organs.

EYE SYMPTOMS AND CONDITIONS IN BRIGHT'S DISEASE.

Read December 19, 1883.

BY WM. S. LITTLE, M. D.

AMONG nine hundred and eleven cases of Bright's disease reported by different observers, changes in the retina have been observed to be associated with the kidney affection in one hundred and eighty-five of these recorded cases; these statistics show that twenty per cent. of the cases of Bright's disease have internal eye symptoms. The statistics have varied with the several observers; the lowest average exhibiting retinitis present in 11.46 per cent., the highest in 30.15 per cent. of the cases of Bright's disease. A more exact and larger average than 20 per cent. can only be derived from a study of a larger number of cases of Bright's disease with retinitis than have as yet been

recorded. The known average of 20 per cent. is sufficient to stimulate observation, and enables us to include eye symptoms among the other various manifestations of the disease under consideration, as they are exhibited by symptoms arising in other important organs and tissues of the body. The recognition of Bright's disease from lesions in the eye, in a case already diagnosed from symptoms in other organs, is not of so much importance for diagnosis, as it is a help in forming a prognosis; nor does the condition existing in the eye demand the therapeutics of special medicine.

A sufficient number of cases, however, are seen in an ophthalmic hospital, having no other symptom of Bright's disease apparent than impaired vision, which the ophthalmoscope shows is due to changes in the retina or of the nerve from disease of the kidney, or it is revealed in cases where vision is good, other ocular conditions being treated, so that in this class of cases the recognition of the lesion becomes important as a means of diagnosis.

The ophthalmic physician will diagnose Bright's disease by means of the ophthalmoscope, almost as frequently among the cases he is called to treat, as the lesion will be found to exist in cases where the eye is thus examined, for additional evidence in already diagnosed cases.

As a pathological change can be seen in the eye-ground during life, as cannot be so well viewed in other parts of the body, the observation of the lesion associated with Bright's disease, or of other diseases, as they are exhibited in the eye, are very interesting and instructive.

Externally, the puffy lower lid is the only symptom, and before Bright's time, it was the first evidence of a more general anasarca, and it was looked upon as a symptom of dropsy, before the kidney was known to be the organ producing the condition. In very rare cases exophthalmus occurs from hemorrhage into the capsule of Tenon, due to the breaking down of the orbital vessels or an excess of serum in the orbit.

With dropsy following scarlet fever, as well as that accompanying the act of gestation and the puerperal state in women, some internal affection of the eye, impairing vision, was known to exist, the same as was later known to be present in dropsy associated with kidney disease, from other causes, in both sexes alike.

Prior to the use of the ophthalmoscope, and even before Bright had connected the lesion in the kidney with dropsy, had amblyopia, or even total blindness been a symptom of dropsy accepted by the profession. In 1836, Bright observed amaurosis to occur early in kidney disease, and to be a pronounced symptom, in some cases, fatal brain complication soon following. Landouzy, in 1849, found albuminuria present in these cases of amaurosis. Türk, in 1850, observed fatty degeneration in the retina and this was corroborated later by Virchow and Heymann. Changes in the choroid as well as in the retina were recognized by Müller in 1857.

The vitreous becomes involved from hemorrhages of the retinal or choroidal vessels, detachment of the retina sometimes occurring; cataract may result, and a general disorganization of the eyeball follow. The contents of the orbit may be involved as already described.

Helmholtz' invention of the ophthalmoscope afforded a means of recognizing the internal parts of the eye; in this disease it was of great value, revealing a picture, to supplant such a vague term as amaurosis. General medicine has derived a large share of benefit from what the ophthalmoscope enables us to see in an eye affected with general disease; the decided and brilliant picture that was presented to view, due to kidney trouble, has perhaps done more to advance the use of the instrument than the study of any lesion in the eye limited to it alone, or in any lesion that co-exists with general disease. In 1856, Heymann made the first observation of the lesion with the instrument, and in 1859, Leibrich presented in a colored lithograph the characteristic picture of retinitis albumenurica, now so familiar; since then the Atlases of Mauthner, Jaeger, Allbutt and Magnus have shown the picture of the lesion in the retina, and optic nerve in all its phases. Mr. Hulke, of London, in 1866, found neuro-retinitis present in the disease. Leber's article in *Handbuch der gesammten Augenheilkunde*, v. 2, p. 572, gives an elaborate account of retinitis albuminurica or nephritica; nor must we be forgetful of the valuable article on "Retinitis in Bright's Disease," by Prof. Wm. Norris, in the work on "Bright's Disease and Diabetes," by Prof. James Tyson.

Retinitis, generally symmetrical, if not always so, appears in all forms of kidney disease, while it is principally associated

with the chronic disease of the cirrhotic kidney. Even though symmetrical, its presence is not always known to the patient; especially is this so, as long as the region of the macula lutea in the retina is not affected, so as to produce a central scotoma; lesions near the nerve and peripheral parts of the retina are not so productive of recognizable scotoma in the field of vision. These cases do not have the restricted visual field and rarely loss of color sense that belongs to optic nerve and brain troubles or contracted field of glaucoma. The symptoms may correspond to those of retinitis from any other cause; flashes of light, color sensations, dread of light, impaired vision, to total blindness.

The ophthalmoscopic examination may only reveal the lesion, the patient being ignorant of it, and not observing the gradual change; such a case I saw recently, the macula not involved, but very rude lesions elsewhere; the case was chronic, and in the last stages of the disease, with a fatal prognosis. The picture of this form of retinitis is a decided one as a rule; when the changes are slight, it is uniform; though at times a differential diagnosis is required from the conditions of the optic nerve and retina due to intracranial disease or to pernicious anæmia.

While the lesions occur generally near the optic nerve and to the temporal side, the nerve itself is not implicated to the degree that it might be expected from the picture it presents; the separate spots in the retina finally coalesce, approaching the disc, and involving it; in other cases they surround the region of the fovea centralis, and are much smaller. They may occur anywhere in the retina, varying in size; the shape is somewhat quadrilateral, but may vary as they progress; they present a white appearance. The tissue near the disc and the disc itself appear striated; spots of hemorrhage are found from rupture of retinal or choroidal vessels, opacities in the vitreous are seen. The state of pigmentation is the latest. In chronic cases the color of the fundus is peculiarly yellow, and the character of the picture is at times very much like the neuro-retinitis of brain disease or pernicious anæmia.

The changes result from the hyperæmia and infiltration of the tissue, fatty degeneration and atrophy producing changes in the blood-vessels with hemorrhage; a fatty deposit in the retinal tissue, the fibres of the retina become sclerosed and pigmentation follows.

The optic nerve is not seriously involved, though gray degeneration may exist, and numerous amyloid bodies may be seen with the microscope. The retina does not always present all the stages described; the kidney disease being amenable to treatment, the retina may not undergo any further change; where it is chronic, a like progression ensues in the retina. Marked lesion at times disappear, leaving only a slight trace. The hemorrhagic state is more severe, the sight not only being lost, but the patient's life endangered.

During gestation the same picture exists, the optic nerve may be more seriously involved. A case recently seen, and having uræmic symptoms, being unconscious two weeks prior to the delivery of the child, it being born dead; had complete atrophy of one optic nerve with retinal lesions extensive; the other nerve partially affected and slight retinal changes, patient almost entirely blind. In another case, with convulsions, only slight changes in each eye near the fovea. In succeeding pregnancies, the conditions may arise again, producing more serious changes in the retina.

The prognosis, as far as the vision is concerned, is serious, when the region of the yellow spot is encroached upon, and yet fair vision may remain after subsidence of the disease.

Can a prognosis as to the duration of the disease or to its fatality be derived from the eye symptoms? Only in the chronic stages of the disease, when retinal hemorrhages are extensive and repeated, the heart being diseased. Traube considered the heart the immediate cause of these retinal hemorrhages; but they exist in cases without heart trouble, and in other diseases of the system, and in intraocular conditions. Brain symptoms soon follow in these severe types of retinitis, in chronic kidney disease a general hemorrhagic condition being developed, or uræmia may ensue. In the acute forms of Bright's disease no prognosis of any value can be formed from the eye symptoms, though severe.

As to treatment of the eye in this disease; what renders the disease of the kidney controllable, is only of advantage to the retina; leeching may be useful if the patient is not too anæmic. For the vitreous opacities, in cases where the disease is under control and the acute condition of the eye abated, electricity is of considerable value, much more so than any plan of medication.

The microscope shows a sclerosis of the retinal fibres, the walls of the blood-vessels degenerated, fatty deposits along the fibres and in the layers of the retina, also in places a pigmentation; the choroidal vessels are implicated as well.

Uræmic amaurosis is rare, Wagner finding one case in one hundred and fifty-three of Bright's disease. Graeffe found two cases in thirty-two cases of albuminuric retinitis; it presents no retinal change that is recognizable.

As yet we cannot answer why the retinal changes occur with disease of the kidney. Does the structure of the retina and its proximity to a highly vascular tissue account for it?

215 SOUTH 17TH ST.

DISCUSSION ON EYE SYMPTOMS IN BRIGHT'S DISEASE.

DR. ALBERT G. HEYL: I desire to direct attention to the relation which exists between the intraocular phenomena observed in Bright's disease and the anatomical arrangement of the intraocular arteries. The latter are divisible into certain areas; thus we have one area composed of the arterial circle of Zinn which supplies the optic papilla; another formed by the arteria centralis retinae which supplies the retina; another composed of short posterior ciliary arteries which supply the posterior half of the choroid; yet another composed of the long ciliary arteries which supply the iris and the anterior half of the choroid; the short anterior ciliary arteries also take part in the formation of this area. Now it is established that abnormal changes in connection with Bright's disease may exist primarily in each of these areas, with perhaps one exception to be mentioned directly. Thus the papillary area may be affected, and a papillitis will be found on examination, the choroid and retina and iris being unaffected. Again, the posterior choroidal area may be affected, and there will be found the phenomenon of subretinal oedema, and also in the choroid the white accumulations so well known in connection with Bright's disease. Both of these forms are considered to be rare; perhaps they would be observed oftener were we more alert. A case reported by me (*Amer. Jour. Med. Science*, October, 1874) of retinal separation occurring in one eye, while simultaneously the other was affected by temporary amblyopia, probably belonged among the affections of the posterior choroidal area. Further, the retinal area may be affected, and the peculiar retinal appearances so well known are found. So far as I am aware, no separate affection of the irido-choroidal area had been described; there seems to be no reason why it should not occur. Of course there are many cases in which the areal lesions exist together, but they are to be looked upon as the result of one common cause, not as the result of a morbid process starting in one area and gradually implicating the others.

In referring to the snow-like patches of the fundus, commonly looked upon as the characteristic intraocular appearance of Bright's disease, I

think that to a certain extent they are to be looked upon as the remnants of intraocular hemorrhage. While hemorrhagic clots formed of normal blood are absorbed without any whitish residuum being left, the case is different where the blood, as it circulates in the vessels, is in a morbid state. Thus in a case of pernicious anæmia, I have observed a number of round red hemorrhagic spots with white centres. On examination ten days later the red coloring matter of the hemorrhages had largely disappeared, leaving as a residuum white masses. Some of these were speckled with red points, arranged in spindle-form like the hemorrhages of the retinal nerve-fibre layer. They were, however, simply the unabsorbed remnants of the round hemorrhagic spots seen ten days before. So in morbus Brightii, the blood is in such a morbid condition that hemorrhagic clots rapidly lose their red coloring by absorption, leaving behind a white residuum which may partially, at least, explain the white plaques seen with the ophthalmoscope.

THE SUPPOSED CONNECTION BETWEEN EAR DISEASE AND KIDNEY DISEASE.

Read December 19, 1883.

BY CHARLES H. BURNETT, M. D.

EARLY writers on disease have shown a knowledge of the fact that alterations in hearing occur in the course of general diseases; as, for example, in diseases of the kidneys. It was supposed by them that the alteration in the functions of the ear, in this form of disease, was due to changes in the auditory nerve. But the results of the more reliable modern investigations tend to show that if an ear disease be due to a kidney disease, the lesion usually occurs in the tympanic cavity and not in the auditory nerve.

Certainly accidents of a hemorrhagic or apoplectiform nature might be expected either in the tympanic cavity or internal ear, in Bright's disease, when we reflect upon the deterioration of the blood, and upon the malnutrition and friability of the vascular system, in the later stages of the malady. Further, as Bright's disease is characterized by a tendency to inflammation, especially in serous membranes, and as the membranous structures of the internal ear or labyrinth belong to this class of membranes, very naturally organic change in these tissues might be looked for in Bright's disease of the kidneys.

However, as late as 1856, Rau, in his "Ohrenheilkunde," published in Berlin in that year, claimed that there was not a solitary

reliable observation at that time on record, in favor of any symptomatic relation between the ear and the urinary organs.

In 1869, Schwartze * reported a case of extravasation of blood into the tympanum, as peculiar to Bright's disease, though of rare occurrence, and in the same year, Dr. G. M. Smith, of New York,† "called attention to the fact that impairment of hearing was at times one of the symptoms of Bright's disease, and a symptom which could not be explained by referring it to uremia."

In 1873, Dr. Roosa, of New York, in his treatise on the ear, refers to an obstinate case of suppuration and pain in the middle ear in a man 61 years old, suffering from Bright's disease. In this case, it was supposed that the effect of the renal disease upon the tympanic vessels, was the cause of the acute suppuration, and it was also supposed that the disease in the drum cavity was originally hemorrhagic in nature. The subject was deemed of enough importance to place a physician on his guard for renal disease in cases of hemorrhage into the tympanic cavity.

It must not be forgotten, however, that there is a purely sthenic form, otitis media hemorrhagica, occurring in subjects entirely free from kidney disease, in which the only effusion is pure blood, the removal of which from the tympanum by paracentesis, is followed by cessation of pain and return of hearing.‡

Again, in 1878, Schwartze § states that serous catarrh of the tympanic cavity, is found in syphilis, heart disease, pneumonia, Bright's disease, naso-pharyngeal catarrh, and apparently may be due sometimes to vaso-motor disturbances. The same author says, hemorrhages into the labyrinthine cavity and the membranous labyrinth occur in kidney diseases.

Also, that extravasations of blood into the tympanum (hæmatotympanum) occur spontaneously with acute inflammations in morbus Brightii, cynanche diphtheritica and in endocarditis verrucosa recens et ulcerosa (op. cit., p. 94).

Dr. Paul Pissot,|| in an inaugural thesis, is disposed to consider three forms of aural disease, which may arise in Bright's disease,

* Archiv. f. Ohrenkellkunde, Bd. iv, p. 12.

† N. Y. Acad. of Med. See Roosa's Treatise, 1873, p. 257.

‡ Roosa, Transactions Amer. Otol. Socy., 1872, and O. P. Pomeroy, Ibid., 1875.

§ Pathological Anatomy of the Ear. J. O. Green's Translation, pp. 97 and 157.

|| These pour le Doctorat en Médecine. Faculté de Med. de Paris, April 4, 1878.

viz.: tinnitus aurium, partial deafness, and complete deafness. His conclusions are, that affections of the hearing may arise at the beginning or during the course of the renal disease. Intermittence seems to be characteristic of these forms of deafness, which may be contemporaneous with the œdema, or may precede it. They appear with all forms of the disease, and are manifested with variable intensity. But he cannot say to what special lesion of the ear, or of the nerve of hearing, their symptoms are attributable. Delacharrière, a responsible aurist, examined the cases upon the history of which the thesis is based, and found rupture of the membrana tympani, abnormal vascularity of the manubrium, and sclerosis of the tympanum, and was disposed to regard the conditions as causative forces. Pissot held that the hypothesis of Rosenstein, according to which there is œdema in the course of the auditory nerve within the cranium, may explain the intermittence and variations of intensity in these morbid manifestations. This latter process may be analogous to the œdema of the glottis and vocal cords noted in Bright's disease, by Fauvel, in 1864. A similar symptom has been noted by Sée.

In alluding to chronic, non-suppurative aural catarrh in children, Von Troeltsch * says: "A greater blood pressure from increased action of the heart, as in Bright's disease, must necessarily produce a certain hyperæmia, even in the mucous membranes of the head."

Albert H. Buck,† of New York, expresses the opinion that in some instances a serous fluid, deeply tinged with the coloring matter of the blood, finds its way into the tympanic cavity through other than inflammatory causes. "Instances of the latter form of disease are very rarely met with, and then usually in connection with a depraved state of the general nutrition, as in morbus Brightii."

Dr. Maurice Raynaud ‡ expresses the opinion that diabetic otitis is not only more frequent than is supposed, but that when this has once become a well-known fact, it may prove a pathognomic index, like anthrax, diffuse phlegmon, and certain erythematous eruptions about the genitals, and arouse suspicion of the presence of the renal disease thitherto unsuspected. He cites a case of

* Diseases of the Ear, in Children. J. O. Green's Translation, 1882, p. 67.

† Diagnosis and Treatment of Diseases of the Ear, N. Y., 1880, p. 164.

‡ Clinical lecture at La Charité, Paris. Annales des Maladies de l'Oreille, etc., Mar. 1881.

well-marked diabetes mellitus, in which there suddenly occurred, one evening, a severe earache, after the patient had been in the hospital two weeks, and most carefully watched, so that no chilling could have been the cause of the ear pain. The pain became intense, and towards midnight of the same evening in which the pain set in, there occurred an abundant hemorrhage from the drum-cavity of the affected ear; which was followed by immediate relief. This was followed for several days by a copious sero-sanguinolent, and then serous discharge, which contained leucocytes, and albumen, as shown by heat, but no sugar. Post-mortem examination, twenty-three days after the attack of otitis, revealed a large perforation in the anterior segment of the membrana tympani, red, fungous and bleeding mucous membrane in the drum cavity, in which there was a pink, purulent liquid. The ossicles were not dislocated, but were imbedded in granulations, and near the stirrup was a clot of blood. The mastoid cavity was filled with a rose-colored liquid, containing pus-cells, and its bone substance was greatly injected and marbled, presenting all the appearances of inflammation of bone tissue. The labyrinth showed no alterations. The author concludes that otitis in the petrous bone is a peculiar and constant symptom of diabetic otitis.

P. McBride,* in an article devoted to the consideration of the various causes leading to aural disease, states that "occasionally the ear is affected in Bright's disease by hemorrhage into the tympanic cavity. The tympanum becomes filled with blood, which probably either becomes absorbed or leads to suppuration. Schwartze was perhaps the first to observe this condition." McBride further says: "I am not aware that sudden labyrinthine deafness in the course of Bright's disease has been described, but it seems probable that such a contingency might be looked for here and also in pernicious anæmia, in which retinitis hemorrhagica is not uncommon."

In the recently published work of Prof. Politzer,† on the ear, the author's experience is that in cases in which a supposed connection existed between the organic renal disease and an aural malady, the fundamental cause lay in very apparent changes in the middle ear. He has also found that "catarrhs of the ear run

* Edin. Med. Journal, February and March, 1882.

† Cassell's English translation, 1883, Philada.

an unfavorable course in tuberculosis, Bright's disease, and all cachexiæ by which the nutrition of the general system has become deteriorated."

CONCLUSIONS.

1. Evidences in favor of either frequent or well-marked aural lesions, dependent upon renal diseases, are extremely meagre.

2. Those lesions in the ear, which have been found in connection with Bright's disease and diabetes mellitus, and which may have been dependent upon the dyscrasia induced by these renal disorders, are in the form of sero-sanguinolent and hemorrhagic effusions into the drum cavity. But the latter must not be mistaken for the sthenic form of otitis media hemorrhagica.

3. From the serous nature of the membranous structures of the labyrinth, organic changes might reasonably be expected in Bright's disease, but positive proof of the occurrence of such lesions based on ante- and post-mortem history is wanting.

NITROGLYCERINE AND THE CHLORIDE OF GOLD
AND SODIUM IN THE TREATMENT OF
ALBUMINURIA.

Read December 19, 1883.

BY ROBERTS BARTHOLOW, M. D., LL. D.

HITHERTO the therapeutics of renal diseases have not advanced in the same ratio as our knowledge of their pathology. It cannot be said now that a cure has been found, but that two remedies of real value are available. My contribution to this symposium, on albuminuria, consists in an attempt to define the place which these remedies should occupy in a curative scheme. To do this, in even the briefest way, I must clear the ground with a preliminary statement

I start with the proposition that those renal lesions united by the common symptom—albuminuria—are of neural origin. There is a kinship between diabetes and Bright's disease. One of these is sometimes substituted for the other; and during the course of some rare cases of exophthalmic goitre this substitution occurs. Irritation of a certain part of the floor of the fourth ventricle is followed by glycosuria; of another part by albuminuria. The recent observations of Da Costa and Longstreth prove that a relation exists, whether casual or sequential, between certain

renal lesions and degenerative changes in some ganglia of the abdominal sympathetic. The hypertrophy of the muscular coat of the arterioles, discovered by Dr. George Johnson, and the increased tension of the vascular system due to an irritation of the vaso-motor centre in the medulla, both present in the chronic forms of albuminuria, are further evidences of the agency of the nervous system. It was, more especially, the condition of elevated tension of the vessels which led to the use of nitroglycerine. This remedy before all else reduces the vascular tension. It also lessens the work of the heart by removing the inhibition exercised by the pneumogastric nerve.

This remedy appears to have been first used by Mr. Robson, an English surgeon, in cases of albuminuria, and by him employed, because the high tension of the vascular system has proved to be so pronounced an element in the more chronic cases. I have, myself, seen some remarkable instances of relief—indeed of cure—effected by it. If time were now available, I could give some striking examples. In cases of mitral disease accompanied by albuminuria, it also renders the highest service—for the diminished peripheral tension lessens the work to be done by the heart, and assists in the more equal distribution of the blood. The effect of this in relieving the renal congestion is obvious.

Chloride of gold and sodium has quite another function. It has long been known that this remedy has a special direction to the genito-urinary apparatus. The ovarian and uterine organs in the female, the testes and vesiculæ seminales in the male, are stimulated by it, and the kidneys, by means of which it is eliminated, and in which it tends to accumulate, are decidedly affected by it in function and structure. In common with some other agents of the class to which gold belongs—for example, corrosive sublimate—the chloride acts on connective tissue and checks its over-production, or its hyperplasia. It would be quite impossible in this note to go over the evidence on these points, and hence I must ask your assent to these statements. They have been accepted as true of gold, from the days of the alchemists and iatro-chemists, as any one may ascertain from that curious collection of mediæval medical learning—the Anatomy of Melancholy. It has happened, strangely enough, that Hahnemann and his followers have profited by this knowledge, and have used

gold preparations—especially *aurum potabile*—in the treatment of renal diseases with success.

How and when are these remedies to be used?

Nitroglycerine is now administered, as all present know, in the form of the centesimal solution—1 minim of the pure drug to 100 minims of alcohol. The initial dose of this one per cent. solution is one minim, which should be increased until the very characteristic physiological effects are produced. The susceptibility to the action of nitroglycerine varies greatly, and hence the dose cannot be stated in advance. It is necessary to produce some obvious effect. To maintain the same level of action, a slight increase in the dose may be required from time to time. As the effect is not lasting, the interval between the doses should not exceed three or four hours.

The administration of nitroglycerine should begin in acute cases immediately after the subsidence of the acute symptoms. It is indicated in chronic cases at all periods, but is more especially useful, if given before hypertrophy of the muscular layer of the arterioles has taken place. When it acts favorably, the amount of albumen in the urine steadily diminishes. The mechanism of its action consists in the lowering of the pressure in the renal vessels. How far any curative effect proceeds from action of this remedy on the sympathetic system, remains to be determined.

Chloride of gold and sodium is indicated in the subacute and chronic cases, especially the latter. The earlier it is given the better, if structural changes are to be prevented or arrested. The good effects to be expected from it will depend necessarily on the extent of the damage already inflicted on the kidneys.

The usual dose is $\frac{1}{30}$ grain, twice a day, but this may be much increased, if necessary. At the outset, $\frac{1}{10}$ grain may be given; in a week the dose should be lowered to $\frac{1}{15}$ grain, and after a month the regular dose of $\frac{1}{30}$ grain should be steadily pursued, with occasional intermissions. Indigestion, gastralgia and colic pains, nausea or diarrhoea, are occasionally caused by it; and if so, the quantity administered must be reduced. It is usually borne without any discomfort, but after prolonged administration, salivation, weakness, emaciation, trembling and other nervous phenomena may occur possibly. Such effects, however, are wanting in my experience.

The treatment of albuminuria by nitroglycerine and the chloride of gold and sodium, does not necessitate the exclusion of other means—hygienic, climatic, or dietetic. These remedies should, however, be given uncombined at different hours, and their actions should not be hindered or obscured by the effects of other agents given with like purpose. To this general statement there may be two exceptions: with nitroglycerine, amyl nitrite or sodium nitrite may be given; with the gold and sodium chloride, corrosive sublimate may be combined. If doubts may be felt in regard to the propriety of depending on the utility of these remedies, they need not be long experienced, for if no good effects are observed in two weeks, they may then be discontinued.

DISCUSSION ON THE USE OF NITROGLYCERINE AND CHLORIDE OF GOLD
AND SODIUM IN ALBUMINURIA.

DR. TYSON: I have no experience with these remedies yet, but shall take an early opportunity to try them. I have hitherto had no success with specific remedies in chronic Bright's disease, and generally confine myself to placing the patient in a situation most favorable for nature's curative operations, and to combatting symptoms; although I have always sought to avail myself of those remedies which reason and experience have suggested as useful. It is quite consistent with the usual views as to the therapeutic action of iodide of potassium that we should expect it to remove the interstitial overgrowth in contracted kidney, but I can now say that I have given it a fair trial, in cases, too, of medical men, where I have had the intelligent co-operation of the patient; but without any result.

So, too, with what might be called specific remedies for albuminuria. I have tried them all as they have been suggested: tannic acid, gallic acid, calcium benzoate, fuchsin, and many others, without feeling justified in inferring that they have any direct action in diminishing albuminuria. It is true that albumen has been shown to decline rapidly while certain of them have been in use, but the cases have always been such in which it would be impossible to show that the albumen would not have diminished under the same circumstances without the use of these remedies. On the other hand, I am satisfied of the good effects of rest and diet upon albuminuria, and have to conclude that up to the present time these are our most reliable measures.

DR. BRUEN: I wish to know if the occurrence of diarrhoea is a contra-indication to the use of nitroglycerine. I have tried it in heart-disease, but had to stop it on account of the diarrhoea.

DR. BARTHOLOW : I have given nitro-glycerine a good deal, but have not had diarrhoea to occur. I have begun with one drop, and increased the dose very much without any sign of gastric or intestinal irritation. This agent is characterized by its power to take off the inhibitory action of the pneumogastric nerve, but its effect is not limited to this, nor to its action on the kidney. It has a general influence on the trophic nervous system. In reference to the treatment of the various forms of albuminuria, we may fall back on the old rule that what is good in one case is good in all analogous cases.

When speaking of sodio-gold chloride, I referred to Burton's Anatomy of Melancholy, in which are numerous allusions to the medical uses of gold. The chemical doctors knew of this use. The effect of gold on the sexual organs has long been known. The rule is that if a remedy has an action on an organ or set of organs, it will probably be beneficial in the disease of that organ. The curative action, of course, is not to be expected where tissue is lost or extensively changed.

NOTE ON SOME RECENTLY SUGGESTED DELICATE TESTS FOR ALBUMEN.

Read December 19, 1883.

BY JAMES TYSON, M. D.

MOST of the members of the Society are probably aware that recently several delicate tests for albumen have been suggested by different observers. Among them are :—1. A saturated solution of picric acid, by Dr. George Johnson ; 2. Saturated solution of ferrocyanide of potassium after free acidulation by citric acid, suggested by Dr. Pavy ; 3. Standard solution of potassio-mercuric iodide, suggested by Tauret,* after acidifying the urine by citric acid ; 4. Equal parts of a saturated solution of sodium tungstate (1 in 4), saturated solution of citric acid (10 in 6), and of water ; 5. Acidified solution of potassio-mercuric iodo-cyanide. The last two were suggested by Dr. George Oliver in the *Lancet*, Feb. 3, 1883. To complete our list of available tests, we may add :—6. The ordinary heat and acid test ; 7. The nitric acid test, by Heller's overlaying method ; and 8. The acidulated brine, suggested by Dr. Roberts, of Manchester, England, and consisting of an ounce of hydrochloric acid added to a pint of water, saturated with common salt, and filtered.

* Mercuric Chloride, 2·7 grains ; Potassium Iodide, 6·4 grains ; Distilled Water, 100 c.c.

The whole subject of these delicate tests has been gone over by Dr. George Oliver, of London, who has published the result of his labors at various times in the *London Lancet*, early in 1883, and more recently in a little volume of fifty-four pages, published by Lewis, of London.

As all of the tests which are not distinctly acid require the previous acidulation of the urine, Dr. Oliver has modified the picric acid solution by adding two drachms of citric acid to one ounce of the solution.

Leaving out the potassio-mercuric iodo-cyanide as a solution troublesome of preparation, Dr. Oliver's results as to all the other tests just named are as follows :

Adopting Heller's method of overlaying the test solution by the urine (all of them as above prepared are specifically heavier than ordinary urines, while the pure picric acid solution is lighter), ONE PART OF ALBUMEN may be detected in

PARTS OF URINE.

TESTS.

20,000	}	by the	{	Iodo-mercuric, Picric, Tungstate.
10,000				
12,000	}	by the	{	Ferro-cyanide, Acidulated brine.
6,000 to				
7,000	}	by the	{	Heat, Nitric acid.

I have carefully repeated Dr. Oliver's testings, and have arrived at results which, while not identical, may be said to be practically the same—except, perhaps, in the case of heat and acid combined, which, used in the manner directed in my book “On the Examination of Urine,” I find decidedly more delicate than the pure nitric acid, and more delicate than the acidulated brine solution. So, also, I do not find the sodium tungstate solution quite as delicate as the picric acid and the potassio-mercuric iodide; but they may be placed in the same category. As the result of this experience, it appears to me that the use of nitric acid may be altogether substituted by the acidulated brine, as a solution at once more delicate and, on account of its non-corrosive properties, altogether more satisfactory to manipulate.

In the *Medical News* for October 27, 1883, Drs. Charles A. Cooke and Ralph B. Watkins, resident physicians in the Bay View Hospital, Baltimore, published a paper on the “Value of

Picric Acid as a Test for Albumen," in which they prove that the urine of persons taking six grains of sulphate of quinia or sulphate of cinchonidia daily will invariably give a precipitate with picric acid, nine or ten hours after the administration. Also, that solutions of sulphate of quinia and of cinchonidia containing $\frac{1}{120}$ grain gave a decided precipitate, and one containing $\frac{1}{180}$ of a grain an appreciable precipitate.

With a view to testing these results, I tested my own urine on a given day with picric acid, and found no response. On the following day I took 22 grains of sulphate of quinia in three doses, taking the last dose at 6.30 P. M. The urine passed at 8.30 P. M. gave a beautifully distinct white line, when overlaid by a pure picric acid solution. So also did the urine passed at 9 A. M. the next day; but that passed at 3 P. M. did not respond. I also found that a solution of sulphate of quinia containing $\frac{1}{180}$ of a grain to the fluid-ounce responded similarly.

I then repeated the experiments with the remaining tests, and found that the potassio-mercuric iodide gave identical results with the picric acid solution, *but that the sodium tungstate and ferrocyanide of potassium solutions did not.* So that we shall have in these two test solutions, and particularly in the sodium tungstate, a test solution more delicate than heat, acid or brine, which is not open to the objection of precipitating quinine in solution.

Picric acid has also been shown by Dr. George Johnson to precipitate artificially prepared peptones. And the same is true of potassio-mercuric iodide and sodium tungstate; and as peptones have been shown to be present in a considerable number of urines, they must be admitted as possible sources of error, the exact importance of which is as yet to be determined. The ferrocyanide solution does not precipitate peptones, but, in common with the others, occasionally precipitates amorphous urates. But these, behaving precisely like the amorphous urates thrown down in the Heller test by nitric acid—that is, they form a smoky cloud rather than a distinct layer, and are easily dissipated by a moderate heat—need not be a source of error. An excess of albuminous urine dissolves the precipitate formed by picric, but this need not be a source of error. It is said, too, that potassium salts are precipitated. But, so far as we know, no other agencies likely to be found in urine can lead to error; so that, if we remember to eliminate quinine as a source of error, the picric

acid solution, acidulated with citric acid, is still an available test of great delicacy, which is further recommended by its cheapness and easy preparation; although, so far as our present knowledge goes, the sodium tungstate solution is the best, and least liable to cause error.

DISCUSSION ON ALBUMEN TESTS.

DR. DULLES: The error caused by the precipitation of quinine by picric acid may be corrected by adding nitric acid, which dissolves the quinine precipitate, but not that of albumen. Early in this year, my attention having been attracted to the articles of Dr. Oliver, of Harrowgate, England, in regard to his "test-papers," I wrote to him, asking for some information about them. In reply, Dr. Oliver has been kind enough to send me a set of these papers, with the request that I would report the result of my experience with them. As soon as they came to hand, I instituted some tests to see whether they could be safely trusted to take the place of the old-fashioned boiling and nitric acid tests for albumen. The result of my investigations was to lead me to conclude that they could not. This opinion rests upon the fact that I found the test-papers to react, and indicate the presence of albumen after I had removed all that was possible, by careful and thorough acidulating, boiling, and filtering. I found that albuminous urine thus treated, and no longer giving a reaction to nitric acid or renewed boiling, did react in the presence of certain of Dr. Oliver's papers, though not of all. Some months later I communicated this objection to Dr. Oliver, who replied with a statement that the process relied on to get rid of albumen was not to be depended upon, and gave his own observations, as follows: "(a). Urines which afforded no precipitation with the test-papers were charged with ox-serum-albumen, and were then boiled carefully, acidulated by acetic acid, and filtered through two thicknesses of Swedish filter-paper. The perfectly clear filtrate afforded a cloud (as when a small quantity of albumen is present) by the test-papers. Re-boiling, however, produced no further opacity, and strong nitric acid afforded negative evidence. (b). Pure serum-albumen was dissolved in distilled water. After boiling, etc., as above, the clear filtrate, in which nitric acid gave no proof of the presence of albumen, produced a cloud by the test-papers, though the latter, of course, afforded no such deposit in distilled water only. (c). Albuminous urine treated in the same way gave precisely the same results." Dr. Oliver concluded that "a very small quantity of albumen can remain in solution, even though the albuminous liquid is subjected to thorough boiling; and though this trace of albumen cannot be discovered by strong nitric acid or by re-boiling, it can be brought to light by the other tests." It would certainly appear from Dr. Oliver's experiments that, in this conclusion, he is right; but there is still an objection to these test-papers, founded upon what is claimed to be their chief merit, namely, their delicacy. I am afraid that a test which may disclose the presence of very minute quantities of

albumen in the urine may increase the number of alarmists and of the alarmed about "Bright's disease." If this objection can be obviated by the diffusion of common-sense views in regard to the true significance of occasional small proportions of albumen in the urine, I think much might be gained by employing this ingenious and handy way of testing, proposed by Dr. Oliver. (The package of test-papers was passed round for inspection among the members of the Society.)

DR. JOHN AULDE: Having given this subject some study and investigation recently, I may be able to add something to what has already been said by the gentlemen who have preceded me. That a saturated solution of picric acid is a delicate test for albumen in the urine, none will question; but there is an objection to it, and the same may be said in regard to nitric acid, namely, that it stains the hands and clothing of the operator. The points in its favor are, that a solution can easily be prepared, and it is safe to handle; but there are chances of error, and unless these are first eliminated, the physician may be misled by this method of examination alone.

If a quantity of albuminous urine is placed in a test-tube, and a single drop of the solution allowed to fall upon it, a distinct coagulum will be formed; but when there is an excess of albumen, agitation of the mixture will cause it to be readily dissolved. If there are peptones in the urine, the addition of picric acid will be followed by a precipitate; and, contrary to the opinion of Dr. Tyson, Gerhardt has frequently observed peptones in urine free of albumen, either as a forerunner or consequence of ordinary albuminuria, while Senator states that peptones exist in every albuminous urine in slight quantities. Another source of error arises from the use of quinine, a substance excreted largely by the kidney, and it has already been stated that a weak solution of the alkaloïd, when brought in contact with picric acid, will show the characteristic reaction, but there are other alkaloids which will act in a similar manner, although I am not able at present to name them. The presence of urates will likewise throw down a coagulum with this solution, but not until after some minutes; but it should be stated that there is a material difference between this and the coagulum formed by albumen. In the case of urates it is crystalline, while that of albumen is granular.

We may conclude, therefore, that the picric acid test is an extremely delicate one, but that it is not decisive, and may be used with advantage only as a method of corroborating other tests, and then only after the chances of error have been eliminated.

It will not be out of place here to call attention to the possibility of laying too much stress on the single fact that there is albumen in the urine, as it has been shown that it does exist in normal urine. The recent work of Dr. Millard, entitled "Bright's Disease," is authority for the statement that in a series of examinations, conducted by French surgeons, the urine of soldiers supposed to be in good health and free from hereditary taint, discovered the presence of albumen in no less than eighteen cases out of one hundred.

DR. LEFFMANN: Undoubtedly, the more delicate a test is, the greater its scientific value. While it is true that undue fear may be excited by

detecting very small amounts of albumen in urine, yet, on the one hand, if albumen is ever an ingredient of normal urine, it is only by these delicate tests that this fact can be established; and, on the other hand, if it is always pathological, the recognition of its earliest appearance will be of much clinical value. In my own experience I have found the glacial phosphoric acid the most delicate and easily applied test.

DR. TYSON: The questions which suggested themselves to Dr. Dulles, have, of course, suggested themselves to me. In speaking of picric acid, I took it simply as a type of a group, and I found that all the urines that gave the reaction with it were from cases showing symptoms of kidney or genito-urinary irritation, such as gravel, mild forms of cystitis and the like. I agree with Dr. Dulles as to the unnecessary public alarm in reference to Bright's disease, but I still think that in these delicate tests we have an important addition to our means of early diagnosis. I recall the case of a gentleman subject to gout who consulted me last spring, because a trace of albumen and a few casts had been found in his urine, which was also of low specific gravity, during an attack of gout. My examination was made after the attack had subsided, and I found neither albumen nor casts. Six months later I re-examined the urine and found a trace of albumen by the ordinary heat and acid test, and also a few hyaline casts. The patient was put upon litheated potash, and in two weeks I examined another specimen. This time I again found no albumen by the heat and acid test, but a distinct white line was revealed in overlaying the urine with a pure picric acid solution and underlaying it with the sodium tungstate. I think it may be fairly concluded from such results as these, that if the more delicate tests had been used in the first instance, I would then have detected the albumen. Again, I do not believe in the existence of a *normal* albuminuria. It is pretty certain that we often find small albuminurias which are of no significance. Such an albuminuria may be harmless and of no significance, but it is still not a normal albuminuria. I am aware that peptones occur in urine, and that these are precipitated by picric acid, and I referred to this fact in my note; but this fact need not necessarily interfere with the utility of these delicate tests after they have been thoroughly studied. The whole matter is now *sub judice*.

I have used Dr. Oliver's papers and find them delicate. I have myself never placed a very high estimate upon bedside testing, preferring to use the solutions at home. They certainly are a great improvement over all previous measures suggested for bedside testing.

NOTE ON THE HYGIENE OF THE KIDNEY.

Read December 19, 1883.

BY HENRY LEFFMANN, M. D.

IT has been said by pathologists that a perfectly healthy kidney is never found in the human adult, and it is certain that the vicious habits of civilized life fall as severely on this viscus as on any other. It holds, as we all know, a vicarious relation to some other excretory organs, especially the skin, and has to bear, therefore, the burden, not only of its own work, but of the frequently interrupted work of its colleagues. The problem of preserving the healthy functions of the kidney falls into two phases: how to maintain the organ itself in good condition, and how to keep its complex excretion of such a character that it shall not be the cause of any interference with health. The two conditions are not wholly inter-dependent; kidney disease of an advanced character may exist without any change in the urine sufficient to give rise to local trouble; while, on the other hand, serious urinary trouble may exist without, so far as we know, marked kidney disease.

As regards the health of the organ itself, we have the testimony of various writers to the fact that one of our most common vices, that of the use of alcoholic liquors, is a cause of injury, and the susceptibility of the kidney to this injury is rather increased by the shock which other excretory organs suffer from our frequent errors in dress and ventilation. Parkes does not give any specific statement in regard to the effect of alcohol on the kidneys, but states that although Anstie and Dickinson thought that the action was not serious, yet it probably is of decided moment.

While the etiology of kidney disease itself is but little understood, we know something more of the causes of derangement in the urinary secretion. This liquid presents us a constitution of much chemical complexity. Its most abundant solid ingredient, urea, is fortunately so soluble in water that it is never the source of any deposit or mechanical interference; but we have, in smaller amounts, two classes of substances of contrary chemical character: the uric acid series, but slightly soluble in cold and acid liquids; and the phosphates, which are less soluble in hot and alkaline liquids; these expressions being used in a general sense. In healthy urine the quantity of water and the acid reaction is such as to maintain these ingredients in perfect solution, but this

healthy balance may be disturbed either by the character of the food taken, the quantity of liquid, or the habits of life. It is not necessary here to speak of over-eating or under-exercise; they have often been presented as causes for systemic conditions which manifest themselves in disordered urine; I wish merely to say a little of the effect of the quantity and character of the neutral liquids taken. In a reasonable number of cases in which urinary deposits form particularly uric acid or urates, I believe the defect is due to the want of drinking a sufficient quantity of water. In our large cities, and especially in Philadelphia, an unfortunate sanitary sensationalism has made a great many innocent-minded people afraid of hydrant water, and they either resist the ordinary promptings of thirst as far as they can, or resort to the moderate drinking of expensive mineral water or alcoholic beverages. A portion of the beneficial effect which many so-called medicinal waters have is due to the fact that patients can be easily induced to drink them freely, which they would not do with the common water-supply. An interesting feature of this phase of the subject is the effect of the continued use of waters rich in solids, especially hard or limestone waters. A general impression exists that such waters tend to the formation of calculi, but I have not found any statistical proof of such effect. The English sanitary chemists, who have studied with great care the effects of different kinds of water, have not been able to establish any relation between the quantity of solids in water and the health of those using it; both distilled water and water rich in mineral matter, especially lime salts, have been used without apparent injury.

It is pointed out in Agnew's Surgery that cases of stone in the bladder are not common in Lancaster county, although limestone water is abundant in that district. It is further stated that this form of disease is more common in Kensington than in other parts of Philadelphia. This fact would be against the supposition that the solid ingredients of the water are the cause, since Kensington is, and has long been, supplied with Delaware water, which, although richer in organic matter, is a decidedly softer water than Schuylkill water.

DISCUSSION ON HYGIENE OF KIDNEY.

DR. TYSON: I wish to add my testimony in favor of what has been said in regard to the deficient use of water. This is partly due to the causes

pointed out in the note, and also to the erroneous teaching in regard to the use of water at meals. Many persons do not use enough water at meals. There is no doubt that by use of water, the urine can be kept so well diluted that the less soluble ingredients, which are otherwise precipitated, may be maintained in solution.

DEATH FROM RUPTURE OF AN ANEURISM OF THE AORTA.

Read December 19, 1883.

BY W. T. TAYLOR, M. D.

F. D. B., a letter-carrier, aged 36 years, came to see me on October 15, 1883, for a hoarseness; his voice at times being reduced to a whisper; a troublesome tickling cough, with an expectoration of frothy mucus; and pains through the left breast, extending sometimes down the left arm. He was slightly oppressed in his breathing after exertion, and generally debilitated.

He had not slept for several nights, and an apothecary had given him a chloral mixture, which did not relieve him, but "produced strange feelings, and uncomfortable dreams," so that he would take no more of it.

On examining his throat I found a bifurcated uvula with the right branch much elongated; this was clipped off, relieving somewhat his tickling cough. There was slight dulness under the left clavicle, with some mucous rales, although the respiration generally was vesicular. The heart sounds seemed distinct, but feeble, and the circulation was regular. His temperature was normal.

The Mist. Glycyrr. Comp. with Ammon. Murias. was given to him for his cough and hoarseness. Tinct. Iodine was applied externally to his throat and under the left clavicle, and the painful parts were bathed with Linim. Chloroform.

He was ordered to drink milk freely, to take beef-tea and easily digested food.

For his insomnia I gave him a tablespoonful at bedtime of the following:

R.

Ammon. Bromid. ℥i.

Elix. Ammon. Val.

Liq. Morph. Sulph. aa. ℥i.

M.

This, however, only caused temporary rest, and had to be repeated frequently.

On October 26, as the hoarseness and cough still remained, I gave him some cod-liver oil and lactophosphate of lime, as a restorative, although I did not consider him tuberculous, for there was no hereditary tendency to phthisis in his family, and he was well developed physically.

I advised him to go out on pleasant days, but not to tire himself with long walks, fully expecting that in a few weeks he would be able to resume his occupation.

The pains which affected his breast and left arm began to extend to the back, between the shoulder and up to the occiput, and became so violent that they could be only partially relieved by bathing with laudanum and chloroform liniment, and the internal use of anodynes.

On November 1, accompanied by his wife, he went to a bathing establishment to take a Russian bath, which some person had advised as being a positive cure for rheumatism. Arriving at the place he disrobed himself, preparatory to being bathed, when, on giving a slight cough, he raised some blood, which the attendant perceiving, he was advised to dress again. This he did partially, but in a few minutes such large quantities of blood issued from his mouth, that Dr. Benj. Lee, who was in the building, was summoned to his relief.

The hemorrhage, however, was so excessive that he died in a few minutes.

An autopsy was made, the next day, by Drs. R. B. Cruice and Alexander, which revealed a rupture of an aneurism of the aorta at the intersection with the left carotid artery, which had opened into the trachea. The heart was of normal size, but its muscular fibres were very firm and large; the pericardium was distended with serum. There was some congestion in the upper part of the left lung, and some adhesions about the aneurism; but no tubercle was seen. The liver and other organs were healthy.

The pressure of this aneurism upon the trachea was the cause of his hoarseness and cough; but being behind the lung, I did not hear the aneurismal thrill, and, therefore, was entirely ignorant of the disease until it was revealed by the autopsy.

AN AUTOPLASTIC AMPUTATION OF THE INFRA-VAGINAL PORTION OF THE UTERUS.

Read November 21, 1883.

BY WILLIAM H. PARISH, M. D.

MRS. ———, æt. 26 years, was admitted to the Philadelphia Hospital in October, 1883.

When 19 years of age she was delivered of her first child. She had five subsequent deliveries, three of these being premature. Her last labor was in December, 1882, and occurred at the full period of pregnancy.

About four years ago, in order to escape from a burning house, she jumped from a third-story window. About three years ago the womb protruded, for the first time, through the vulvar orifice. She has been frequently intoxicated.

Menstruation had returned since the last labor, was profuse, recurred too frequently and was attended with pain. She suffered with constipation and frequent urination. Lumbar and pelvic pains were almost constant, and a desire as if to expel something from the vagina of frequent occurrence.

Inspection showed the cervix extending about one inch external to the vulva. There was a deep, bilateral, transverse laceration of the cervix. The anterior and posterior lips were widely separated, and, in appearance, recalled very forcibly the plate in "Barnes' Diseases of Women," stated by Barnes to represent an eversion of the mucous membrane of the cervical canal, but which must have been taken from a lacerated cervix. The cervix was thickened, and when the two lips were approximated presented a bulbous appearance.

The sound entered four and one-half inches into the uterus. A finger in the rectum aided with a sound in the bladder, showed the *fundus uteri* to be at about its normal height in the pelvis, and attached by adhesions to the lateral pelvic wall. The vagina did not protrude outside of the vulva, but, on the reverse, a finger could be introduced nearly the normal distance before reaching the posterior vaginal roof. Anteriorly the finger entered a shorter distance. The portion of the uterus below the attachment of the anterior vaginal wall measured nearly two inches in length. There was neither cystocele nor rectocele.

It was evident that the condition was one of hypertrophic

elongation of the infra-vaginal portion of the cervix uteri, with a transverse bilateral laceration. The elongation of the neck and the position of the fundus constituted what Virchow termed "Prolapsus, without descent of the fundus"—a condition to which the term prolapsus is entirely inapplicable. None the less inapplicable is the term procidentia. An elongated uterus will shorten to some extent after a laceration has been remedied by the operation usually performed for that condition. But I did not deem it possible that such a degree of lengthening as existed in this patient would be materially lessened by the operation.

I concluded to amputate the cervix below the attachment of the anterior vaginal wall. The perineum had been torn off as far as the sphincters ani, and the tonicity of the remaining portion had been greatly lost.

The method of amputating the cervix, adopted by the late Marion Sims, is very liable to be followed by undue contraction of the cervical canal. Amputation with the cold wire of an *écraseur* is even more likely to produce this objectionable result. The hot wire produces a cicatrix peculiarly liable to contract and to bring about most troublesome narrowing of the cervical canal. After amputation by either of these methods a subsequent operation for the remedying of the resulting contraction is often demanded.

I decided to dissect from the cervix its external mucous membrane to within one-quarter of an inch of the point where it is reflected from the uterus anteriorly; then with the bistoury to amputate the cervix, and then to attach with sutures the circular flap of the external mucous membrane to the mucous membrane of the cervical canal. The dissection of the mucous membrane from around the cervix was easily effected, and without troublesome bleeding. The flap thus made was about one-eighth of an inch in thickness, was of sufficient vascularity to ensure its vitality, was circular in form, and was turned up before amputating, as is done in circular-flap amputations of the forearm.

The muscular and internal mucous portion of the cervix were amputated by two strokes of the bistoury, carried respectively from before downwards and backwards, and from behind downwards and forwards. The portion of the cervix thus removed measured one inch and three-quarters. There was no undue bleeding, and it was not necessary to resort to ligation or any

other method of controlling or preventing bleeding. The circular flap was retrenched sufficiently so as with it to merely cover the end of the stump and to be attached to the mucous membrane of the cervical canal without being made too tense and without bagging. The two mucous membranes were then stitched together with silk sutures, six in number. Two additional silver sutures were so introduced laterally as to press the flap well against the stump; these were carried into the muscular portion of the stump. In the after-treatment no tampon was used, as there seemed no tendency to bleed, and the hospital interne was so accessible. There was retention of urine for a few days, necessitating catheterization. No hemorrhage occurred, no special rise of temperature, no indication of undue inflammatory action; in short, no untoward symptom. Three of the sutures were removed on the sixth day. On the eighth day menstruation returned prematurely, and the remaining sutures, including the silver wires, were not removed until the fourteenth day.

The union of the two mucous membranes was perfect, and the stump was completely covered with mucous membrane. The newly-made os was patulous. Seven weeks after the operation the lower end of the uterus was two inches up the vagina, and the orifice of the cervical canal was duly patulous, with rugose borders, but without eversion of the mucous membrane of the canal. The patient was free from her former distressing symptoms.

The perineum had regained much of its tonicity, and I do not think it will be necessary to perform an operation for its restoration. I, however, advised the patient to keep herself under observation, as the latter operation might yet be indicated. She inquired in reference to future possible child-bearing. I told her that there was nothing in her condition subsequent to recovery from the operation, to debar conception, with the continuance of pregnancy to the full period, and delivery safe to herself and child.

The condition presented by the patient prior to the operation is a somewhat rare one, and the operation as performed by myself is not such as is usually resorted to for its relief. The operation calls for, perhaps, greater expertness, as an operator, than do other methods of amputating the cervix, but it leaves the cervical canal in a condition not liable to subsequent narrowing, and in this consists its superiority.

THE BACILLUS TUBERCULOSIS AND THE ETIOLOGY OF TUBERCULOSIS. IS CONSUMPTION CONTAGIOUS?

SECOND COMMUNICATION.

Read November 14, 1883.

BY H. F. FORMAD, B. M., M. D.,

Lecturer on Experimental Pathology and Demonstrator of Morbid Anatomy in the University of Pennsylvania; Mütter Lecturer in the College of Physicians of Philadelphia.

GENERAL CONSIDERATION.

A LITTLE over a year ago* I had the honor of presenting for your consideration some anatomical points in refutation of the etiological relations of the bacillus tuberculosis.

At that time I announced some original observations regarding the histology of scrofulous tissue, tending to place the question of heredity in tuberculous disease upon an anatomical basis. These peculiarities of scrofulous tissues, I submitted as elucidating the etiology of tuberculosis, showing that the peculiar histological condition of the individual, under the influence of simple irritants, and not the character of the irritant, is responsible for tubercular inflammation. It gives me pleasure to state that these observations have since been confirmed by several competent histologists, whose articles on this subject will soon appear in print; besides which, a general interest has been manifested by favorable comments both in America and abroad.

Shortly before the publication of these observations, Koch, of Berlin, had brought forward the discovery of the now famous bacillus tuberculosis, affirming it as the sole cause of pulmonary phthisis, and other forms of tubercular disease, and claiming for it, besides exclusive pathogenetic properties, special morphological and chemical characteristics.

In my first paper, I denied some of these propositions upon grounds of personal investigation, and, subsequently, Koch's researches were also severely criticised by a number of other observers.

* Studies from the Pathological Laboratory of the University of Penna. xi; "The Bacillus Tuberculosis and some Anatomical Points which suggest the Refutation of its Etiological Relation with Tuberculosis, by H. F. Formad." Read before this Society, October 18, 1882.

As interesting and valuable as the discovery of Koch is, from a biological standpoint, its practical value is decidedly overestimated and has, in my opinion, not nearly the significance for medical science which the enthusiastic followers of Koch ascribe to it. The influence of the discovery was, however, great in strengthening the traditional and unwarrantable belief in the contagiousness of phthisis, as held by a small part of the profession and community. On the other hand, this belief led to the popularity of the discovery. In this respect the bacillus theory has perhaps been harmful, and, taking the consequences into consideration, we cannot accept such a theory without the closest scrutiny.

Two practical benefits may accrue from this discovery. The first is that the fear of the effects of the bacillus may induce greater cleanliness in hospital management and enforce improvement in hygienic matters in general. It is doubtful whether the removal and prompt destruction of the sputum would have any influence in checking the spread of phthisis, as the disease is found as often, if not oftener, in the clean palaces of the wealthy as in the unclean huts of the poor. The second benefit resulting from the bacillus theory may be that physicians may become induced to make more use of the microscope in diagnosis; yet, in this respect, the general use of the microscope is hardly practicable on account of the thorough technic and experience required.

To-day, while the bacillus is acknowledged as a common morphological concomitant of tubercle, the pathogenetic properties are denied it by the best pathologists and clinicians, on account of a want of sufficient confirmation of the evidence thus far offered.

The followers of Koch's theory are, however, numerous, but they are recruited largely from the ranks of clinical teachers, book-writers, and others possessing no opportunities for personal investigation.

It may be well to state that, upon my visit to Koch last summer, made with the purpose of doing justice to this important question, I was gratified in many respects. I found Koch an earnest and conscientious worker, and not as dogmatic and extreme in his views as would appear from his writings; nor is he as self-satisfied and as rash to jump at conclusions as are

some of his followers. Koch has the co-operation of an excellent staff of assistants, all able mycologists; but it was a matter of surprise to me that there was not a single competent pathologist connected with Koch's laboratory; and such services are evidently much needed to give to the observations made there the proper interpretation from a biological and anatomical standpoint. I was also pleased to learn in Berlin that the discovery of the bacillus was exaggerated, not so much by Koch himself as by the Imperial Board of Health, which employs him, and by his over-zealous followers in the profession.

There is strong evidence, however, that Koch's investigations are biased by the determination to find for each specific disease a specific fungus.

Following out the various phases in the study of tuberculosis, I am sorry to see that the entire subject is now being considered from a purely etiological basis with reference to bacteria, while the study of the anatomical and biological relations is wholly neglected.

I admire the beautiful bacteridian discoveries of Klebs, and particularly those of Koch in connection with the etiology of tuberculosis. The accomplishment of these results is a triumph for scientific botany; but these studies are much too one-sided to have an application to scientific medicine. The bacillus is there! It is concomitant with most tubercular lesions. It is diagnostic of tuberculous change. It is, on account of its irritant properties, one of the causes of tuberculosis. But this forms no reason for asserting that tuberculosis should be considered a contagious disease, without further investigation and proofs. A contagious disease can have only one cause. I cannot agree with those who define the predisposition to phthisis as being a condition of the organism which offers a favorable soil for a tubercle bacillus. Nor can I believe that inheritance is explained by subsequent infection from cohabitation; *e. g.*, that children become scrofulous by living with consumptive parents.

The latest fruits of the bacillus studies have even inspired Baumgarten (*Centralblatt. f. d. Med. Wis.*, Aug. 4, 1883) and several others to come to the conclusion, in reference to the hereditary nature of tuberculosis, that the bacillus is transmitted in its larval state from mother to foetus in intra-uterine life! One would think, however, that one of the most wonderful

effects of the tubercle bacillus is manifested by the change it produces in the reasoning powers of some of our pathological and clinical investigators, both at home and abroad.

A number of scientific physicians are so delighted with Koch's discovery that their thoughts are more engaged in the bacillus than in the patient infested by it; and some of the younger pathologists are even affected by a regular fanaticism for bacterian studies in tuberculosis. These studies now take the place of their former excellent pathologico-anatomical studies. Consideration is no longer given to the tissue changes, or the nidus which invites the bacteria and nourishes them. In fact, Koch's followers in their enthusiasm exaggerate matters, and, to Koch's own amusement, go further in their bacillus speculations than he himself thinks justifiable. It is really painful to read how some of the younger German pathologists, and a few of the prominent English surgeons, under the influence of the bacillus craze, will make, in their publications, assertions entirely unwarrantable. They describe, for instance, with the greatest ingenuity and exquisite minuteness, how "one or more bacilli" will produce certain histological changes in the lungs or in the peritoneum, designating the exact route to the same; how the different cells, the lymphatic and the blood-vessels are affected; how the bacilli convert one variety of cells into another; how they manufacture giant cells and cheesy material; how acute and chronic phthisis is produced by the bacilli, and the quantity necessary for each; how tubercles develop only and exactly in those places where the bacillus becomes lodged; how, if bacilli alone are inhaled, miliary tubercles form; and how, if the bacillus is accompanied by some other irritants, a broncho-pneumonia will ensue.

All the above statements are made by scientific medical men and pathologists, and offered as broad facts, in full earnest! I only have to say that here evidently observation is substituted by imagination, solid science by speculation; and all this is done for the sake of the convenience in explaining a disease by pretty hypotheses.

The only men who attempted to repeat Koch's experiments, besides the work done in the pathological laboratory of the University of Pennsylvania, were Spina and Watson Cheyne. Of the latter two scientists, Spina came to results entirely different from those of Koch, and they disprove beyond doubt

some parts of Koch's hypothesis. From an analytical and critical point of view, Spina's studies of tuberculosis are excellent, but the technical part of his investigation is deficient, and hence not satisfactory; still, he was more sincere than Cheyne. Watson Cheyne, to whom the "British Association for Advancement of Science by Research" had entrusted the investigation of tuberculosis, and the testing of Koch's researches, did not do justice to his mission. From Cheyne's report (*The Practitioner*, April, 1883) it is seen that he made no earnest attempt to study the nature of tuberculosis, because all he did was to study and experiment with bacteria met with in tuberculous lesions. He went to see some of the different mycologists, consulting only believers in the germ theory; obtained some French and German bacteridian material, and, after testing the same, he reports with great emphasis that Koch's bacilli are a more genuine tubercular virus than Klebs' or Toussaint's micrococci. He did not inquire, nor did he care, whether tuberculosis may have any other cause! He simply imitated some of Koch's experiments with the bacillus material in rabbits and guinea-pigs (only), and obtained, of course, the same results. Furthermore, he made some control experiments, which, as I will show, pass for naught, as they are much more deficient in accuracy than those of Koch.

There are a number of excellent studies in reference to the occurrence of bacilli in the sputum and in tuberculous tissues; but the main part of Koch's hypothesis, *i. e.*, the etiological relation of these bacilli to tubercular disease, remains still unconfirmed.

My own researches on tuberculosis were made from a standpoint different from that of Koch, and they were undertaken five years ago, being carried on continuously since that time by myself and assistants.

My object was to study scientifically the natural history of the disease, without being influenced by any preconceived views. While due attention was paid to external agencies in the production of tuberculosis, the part played by the animal or human organism itself, the behavior of its component cells, and the primary changes in the tissues, were not lost sight of.

I may state that I was fortunate enough to be able to utilize the material of over four hundred cases of tubercular disease

from the autopsy table, including a number of cases studied in the pathological institutes in Europe at various times.

My present research on tuberculosis with special reference to the bacillus question, was carried on during the last year and a half, under the auspices of the Provost of the University of Pennsylvania, Dr. Wm. Pepper. This communication should not be considered a report on my investigations, as these are not yet concluded; but a detailed report of these investigations will be made next summer. Some of the positive results achieved will, however, be referred to in the present paper; otherwise it merely embodies a general critical survey of the question of the etiology of tuberculosis, based upon a careful perusal of the literature of the subject and upon personal observation.

I may state at the outset, that, while the results of my observations force me to-day to make some concessions to Koch, namely, that his bacillus, on account of its irritative properties, can produce tuberculosis under certain conditions, I am firmer than ever in my former views, from the results of repeated observations, that tuberculosis may arise from other causes. The bacillus may be one of the causes, conditionally, but it is not THE cause. The question of predisposition stands in the way of the acceptance of the bacillus theory. Furthermore, I will try to show that tuberculosis is not a contagious disease, and it is particularly in reference to this that I am glad to bring the present subject before the Society, desiring to profit by the discussion which is to follow as a result of the experience and the clinical observation of the individual members of the Society.

The question of the contagiousness of phthisis is one of supreme importance, not only from its scientific, but also from its social aspects.

For convenience in treating the subject of the etiology of tuberculosis, I shall speak of it under the following headings:

1. The definition and the anatomical character of tubercular lesions, including pulmonary phthisis.
2. The predisposition; the predisposing conditions; scrofulosis.
3. Tuberculosis without predisposition due to inflammation of serous membranes.
4. Question of contagiousness; clinical aspect.
5. The bacillus tuberculosis.

6. Experiments—"pro" and "contra;" traumatic tuberculosis.
Conclusions.

All these considerations will have to be, of necessity, very brief.

I.—THE DEFINITION AND THE ANATOMICAL CHARACTER OF TUBERCULAR LESIONS, INCLUDING PULMONARY PHTHISIS.

No definite understanding concerning a disease can be arrived at unless some fixed conception of the anatomical characters and various expressions of the lesions of that disease is formed. Thus, as regards the question of tuberculosis and pulmonary phthisis, the matter would be much simpler if a general understanding could be arrived at as to the definition of tuberculosis and phthisis in its different anatomical manifestations. The pivot of the question is, what to call a tubercle, or a tubercular lesion.

The traditional conception of a tubercle being a miliary node, the belief is that nothing is tuberculosis unless expressed by nodes, and that everything is tuberculosis that appears to the eye as containing nodes. These misconceptions are what bring the confusion and prevent the settlement of the question of tuberculosis, both at the post-mortem table and in the hands of the experimenter.

One of the results of this confusion is that some clinicians divide pulmonary phthisis into catarrhal, cheesy, fibroid and tubercular proper, because they do not see tubercle nodules in some of these forms of phthisis. They seem not to be aware of the fact that miliary tubercles do not belong necessarily to the picture of pulmonary phthisis; and, on the other hand, that those nodes which occasionally appear as miliary tubercles are not miliary tubercles at all, but are only miliary foci of broncho-pneumonia, due to aspiration, as will be explained later. Miliary tubercles, if at all present, usually form a part of a general disease, a tuberculosis of the whole body. In rare instances, when the miliary eruption takes its departure from the lung, the miliary nodules may be limited to the lung.

A more serious matter is the mistake that experimenters make of interpreting as tubercles the so-called inhalation tuberculosis, artificially produced in animals by means of a spray with tuberculous and other matter. The nodules produced in the lung under these circumstances are not miliary tubercles—in fact, no

tubercles at all. They are simply miliary broncho-pneumonic foci, limited to those terminal collections of air-vesicles, called acini, in which some of the inhaled irritative material became lodged. The natural round boundaries of these acini correspond exactly to the usual size of miliary tubercles, and appear as such even under the microscope, although filled merely with an unorganized inflammatory exudate. The uniform distribution of these foci is due to the fact that the inhaled irritating particles are distributed only to individual and the most accessible bronchioles and acini, thus simulating a true miliary tuberculosis of the lung. Similar broncho-pneumonic foci occur in the human lung from self-aspiration of tuberculous material from a primary focus to some other portion of the lung or throughout the whole lung. This was proven long ago, but the inhalation experimenters appear not to be aware of that fact. Careful personal observations and experiments, to be recorded in my forthcoming report, have convinced me that such inhalation experiments prove nothing, either for or against the contagiousness of tuberculosis, in connection with which they have been brought forward as the strongest affirmative proofs. Furthermore, it must also be remembered that the so-called experimental inhalation tubercles, as a rule, remain local.

On the other hand, miliary nodes or tubercles are met with, not only in tubercular lesions, but also in a variety of similar and dissimilar lesions, such as pearl disease or bovine tuberculosis, lupus, leprosy, glanders, actinomycosis, chancre and gommata, cancer, typhoid infiltration, lymphomatous and leukaemic lesions. All these lesions, even cancer ("miliary carcinoma"), are able to give rise to exquisite miliary disseminations, or eruptions, although these are most frequently observed in tuberculosis. We already recognize leprous, lupous, glanderous, syphilitic and other tubercles, in contradistinction to tuberculous or scrofulous tubercles.

To the above nodular formations may be added a variety of minute inflammatory foci of granulation tissue, organized around minute foreign bodies introduced experimentally into various tissues; also, "false tubercles," such as mere unorganized collections of lymphoid cells, held together by some fibrine or by some artificial or natural round boundaries, such as is the case with the referred to "inhalation tubercles;" and further, also, the eruption and follicular enlargements in the skin and mucous membranes.

The question now arises, how to distinguish between these various kinds of nodules, apart from their clinical features. They may all undergo a cheesy or a fibrous change, may calcify, and may contain giant cells. In all, bacilli may be found if a cheesy change occurs, or tends to occur.* Without desiring to appear skeptical, I must say, however, that it takes the skill of a Koch to differentiate sometimes the bacilli met with in the various kinds of nodes, even after applying all micro-chemical tests.

The true tuberculous tubercles occasionally do not show any bacilli whatever, as I will prove from personal observation, and from the reliable testimony of others. It will also be shown that the only test now left for determining the pathogenic peculiarity of tubercle—namely, the asserted exclusive property to produce tuberculosis—is conditional and uncertain, since substances, not tuberculous, may, under similar condition, have the same effect.

Therefore, it is impossible to define tuberculosis, either by its anatomical peculiarity or by the pathogenic property of its nodes.

Another important point in the natural history of tuberculosis is the cheesy degeneration of its products; but here, again, we are surrounded by difficulty if we take only the cheesy product into consideration, because all the lesions mentioned before as being characterized by, and as being capable of, nodular eruptions, have the tendency to undergo cheesy change. Besides this, simple inflammatory products have been observed to undergo a similar change, as is instanced by that form of cheesy hepatization, sometimes following croupous pneumonia, and also by certain forms of rapid necrotic changes, such as occur in acute septic inflammations, designated lately by the name of coagulation necrosis. It must, however, be remembered that the total absence of cheesy masses in the body of tuberculous subjects has been observed.

To tell tuberculosis from allied lesions is only possible after a consideration of the soil in which it develops, and the location of the products, together with the clinical and anatomical manifestations.

What is the origin of tubercle nodules?

The primary occurrence of miliary tubercle nodes is, to my mind, very questionable. I have never seen it occur without

* Save in cancer and in leukæmic formation, for which the proof of their etiological relation to bacteria is put in near prospect by some of the too ambitious germ theorists.

the co-existence of diffuse granulation tubercle. This granulation tubercle is recognized by all as being a simple inflammatory granulation tissue, characterized by cells somewhat larger than ordinary lymphoid cells, containing usually giant cells, but undergoing very readily cheesy change on account of its deficiency in blood-vessels. This tissue is regarded by most pathologists as secondary to miliary tubercles; but I think, after careful observation, that the reverse is the case; because I have never seen upon the post-mortem table, or in animals, *primary* miliary nodes without the granulation tissue, while the granulation tubercle tissue does exist very frequently without the nodes. Moreover, *primary miliary* tuberculosis is unknown.

That tubercle is primarily a simple granulation tissue of inflammatory origin has been proven experimentally. E. Ziegler (Centralbl. f. d. Med. Wis., 1874, No. li) made the following interesting experiment: He inserted below the skin or into the peritoneum of animals, a number of pairs of glass covers, each pair glued together in such a manner that between them there existed an interspace just large enough to allow the entrance of white blood corpuscles; and these corpuscles, not being severed from the body of the animal, then formed a tissue between these plates of glass, which, upon removal after various periods, could be readily examined under the microscope, and the conditions of tissue formations traced. Under these circumstances it was observed that whenever blood-vessels had developed in the new-formed tissue between the glass plates, an organization of the cells into a perfect connective tissue took place; but, when the formation of blood-vessels had failed to occur, then a tissue simulating tubercle tissue was formed, made up of epithelioid and giant cells, and cheesy changes had occurred. Ziegler very properly declared the latter product to be tubercle tissue. I have had, and have at present, ample opportunity to corroborate the accuracy of these observations. Ziegler's experiments were repeated in the pathological laboratory of the University of Pennsylvania, by Hammer, and at present are being carried on by Woodnut. By these experiments, made with slight modification, after the method of Ziegler, under varying conditions and upon various animals, it was shown that the granulation tissue gradually gave origin to tubercle nodules. Furthermore, these experiments showed that the tubercle nodes and cheesy changes

ensue without the action of bacilli, as the latter were found not to be present when proper care was taken, during the execution of the experiment, to exclude them.

From the examination of tubercular tissue from various sources, I may say that I have seldom succeeded in finding tubercle bacilli in newly formed tubercular tissue made up of small lymphoid cells. In older tubercular tissue, made up of opaque epithelioid cells and giant cells with a nodular arrangement, particularly when this tissue is undergoing necrotic change, bacilli are quite common, except in some forms of tubercles of serous membranes, to be referred to later. Tubercle tissue that has undergone a complete cheesy change, contains the greatest number of bacilli. Cheesy matter of any source is a dead substance, and it is usually inhabited by bacilli, if these get access to it; while other bacteria are scarce on this nidus.

Examination of materials from the autopsy table shows that tubercle expresses itself in various manners. Primarily, tubercle occurs as a mere infiltration of lymphoid cells in the adventitia of blood-vessels, or as small nodular masses of lymphoid infiltration around blood-vessels or ducts of any kind; or tubercle tissue may organize within blood-vessels and various ducts. Sometimes tubercle appears as a diffuse lymphoid infiltration, extending over a larger area, showing a greater or less tendency towards the formation of nodes and cheesy or fibroid change, as in the lungs. Tubercle tissue may form masses of the size of a hen's egg, particularly in the brain and serous membranes. In the lungs, in racemose glands, and in mucous membranes, catarrhal changes always follow the tubercle infiltration. On serous surfaces primary tubercles appear often as flat or nodulated patches of various sizes (in peritoneum), or as fungoid vegetations (in synovial cavities), or even as large plastic masses (in omentum). In the skin and mucous membranes, tubercles produce eruptions, ulcers or nodular indurations; in bones—caries, with abscess formation in surrounding parts (cold abscesses). Fibroid capsules, made up of connective tissue, due to reactive inflammation, enclose often smaller or larger tubercular masses, especially if these have undergone cheesy change.

Primary tubercle manifests itself quite variously in different animals. In guinea-pigs and rabbits, it appears mainly as small cellular infiltrates; in dogs, it often undergoes a fibroid change;

in goats, and especially in cattle, tubercle often forms large nodular, sometimes pedunculated masses which often calcify;* in birds it forms, preferably in the liver, large round mulberry masses, which, on section, appear sometimes as horny radiating structures.

Secondary tubercle presents an aspect entirely different from primary tubercle, and it manifests itself in nearly all instances in but one form, namely, as a fine miliary eruption representing those well-known gray semi-transparent nodules of the size of a millet-seed, called miliary tubercles. These seem to lie in the perivascular lymph-spaces, and are probably distributed throughout the body mainly by means of these lymph-channels of the blood-vessel walls. Tubercles do not occur in avascular tissues. There is, however, a second form of embolic or metastatic tuberculosis which evidently distributes itself by the blood-current proper, and it appears in the form of conical masses or round nodes which may reach the size of a walnut and are located usually at the bifurcation of arteries. No mention of this form of tubercle is made in text-books, although upon the post-mortem table this variety of tubercle is a very common occurrence. Especially is it seen in the lung and, more rarely, in the spleen and liver.

Taking into consideration the enormous frequency of local tubercular lesions (counting pulmonary phthisis into this category), the occurrence of secondary or true miliary tuberculosis must be considered a rare affection. A tuberculosis affecting the lining of even the whole peritoneal cavity, including its lymphatic glands, or that of the pleural sacs, or that involving one or both lungs, must, when occurring thus in but one locality, be considered a local tuberculosis. In such instances, the tubercle spreads by continuity of structure.

It is a fact, established by Virchow, that tuberculosis is at first a local disease, and only becomes generalized secondarily. This generalization does not affect the blood like in infectious diseases, but it takes place simply as an embolic process like in some tumors. Local tuberculosis in external organs and accessible lymph-glands is often a harmless affection. It is strongly

* I have met with, on the autopsy table of the Philadelphia Hospital, two cases of tuberculosis in man that were identical in every respect to bovine tuberculosis. Dr. Creighton, of Cambridge, England, describes a number of cases from his own observation, and collected from literature. *Bovine Tuberculosis*, London, 1881.

related to primary tumors. Complete early removal of local tubercular lesions is practiced successfully in Europe. Volkman and others have removed, for instance, lymphatic glands, testes, and joints affected with fungoid synovitis, with the object of preventing secondary tuberculosis, and have thus prevented a general miliary tuberculosis.

Nor should a gloomy prognosis be given in early phthisis. It is astonishing what a large number of healed cavities and cicatrices in the apices of lungs are found on the post-mortem table, indicating the healing of phthisis in persons who long subsequently died from some other causes in later life.

We have seen that tuberculosis manifests itself quite differently as to structure, appearance, distribution and termination, in the various animals, and even differently in the various organs of one individual. Our studies have shown that these variations in the expression of tuberculosis depend upon the structural peculiarities of the various kinds of animals, and sometimes even upon the difference of the structure in animals of the same species. We have also seen that even in human beings tubercle tissue may manifest itself in various forms. In some individuals it develops rapidly, and spreads over large areas, becoming generalized and undergoing speedy cheesy change; in other individuals it develops slowly, fibroid change predominating; and in others the tuberculous product may calcify. In most individuals tubercular lesions may remain entirely local.

It is well known from clinical experience that the general condition of the organism has very much to do with the healing of a local tuberculosis. A local tuberculous inflammation may heal or become arrested in its progress, if the patient "gets strong," or it becomes more developed and aggravated if his general health "runs down." Observation has further shown that any simple non-specific wound in a weak, ill-nourished individual, may fail to heal, becoming unamenable to treatment, and probably assuming a tubercular character.

In some animals spontaneous tuberculosis is unknown, and while some animals are easily tuberculizable experimentally, in others tuberculosis cannot be produced.

It is in accordance with experience that in a large number of families the predisposition to tuberculosis is hereditary, and that their members die promptly of phthisis at a certain age from the

effects of a simple "cold," while in the history of other families this affection is unknown. Every individual is liable to acquire syphilis, small-pox, and other contagious diseases, but it is proven that not every one can have tuberculosis. A special predisposition and a special individual are required. In such an individual a simple inflammation, resulting from any cause whatever, can produce tuberculosis.

Therefore, for the development of tuberculosis, two conditions are necessary :—

1. A *definite* soil.
2. An *indefinite* irritant.

The reaction of the soil is always the same under the influence of any irritant, whether that irritant be a bacillus or not; since the result (tuberculosis) following a lesion in such a soil depends upon the character of the soil and not upon the character of the irritant, even though one irritant, say bacilli, may act more readily than other irritants.

In view of the demonstrated fact that simple injuries of any kind can excite a tuberculosis, but only in certain individuals and tissues, it is evident that tuberculization is determined by the kind of soil and not by a specific irritant. *Tubercle should therefore be defined as being an inflammatory new formation in a specific individual or tissue.*

What is the place for tubercle in pathology? The anatomical criterion for tubercle is a granulation tissue made up of lymphoid or epithelioid cells, which, on account of deficiencies in the soil, does not undergo any higher organization, nor tend to heal; but tends to form nodes and undergo cheesy change. Under favorable circumstances it may heal through fibroid change. The elements of tubercle tissue may spread by continuity of structure to surrounding parts, and occasionally tend to the production of metastasis, distributing themselves by means of the lymphatic system principally, and rarely by blood-vessels; and may generalize themselves through the whole body, forming miliary nodes or tubercles.

This miliary eruption of tubercle appears to have the same relation to the primary tubercular growth as the secondary metastatic cancer eruption has to the primary cancerous growth. Like in cancer, the elements of tuberculosis may be arrested by the lymphatic glands governing the affected region.

In tuberculosis, lymphoid cells form the nodes; in cancer, epithelial cells. While secondary cancer nodes are, as a rule, much larger than tubercle nodules on account of the well-known great proliferating power of epithelium, it is also a fact that cancer may appear as a miliary carcinosis, expressed by minute nodules not distinguishable microscopically from miliary tuberculosis. Cancer is proven to be a local disease. It is not contagious. It is infectious only to the individual who is affected by it; *i. e.*, it is self-infectious. And so is tubercle, in every respect, a local, self-infectious disease.*

That local manifestation of tuberculosis in the lung, which is designated by the traditional name of pulmonary phthisis, forms perhaps nine-tenths of all tubercular lesions, and hence deserves some special consideration.

I arrange myself with those who regard all forms of pulmonary phthisis as tubercular. There are only three or four lesions of chronic wasting disease of the lung which may be excluded from phthisis. These are atelectasis, or collapse from pressure of effusions; bronchiectasis, in which the enormous dilatation of the bronchi may lead to large cavities and atrophy of lung structure; primary fibroid changes; and abscess of lung. Yet all these conditions may become tuberculous from secondary inflammatory changes which usually follow.

The lesions that are known as catarrhal pneumonia, bronchopneumonia, pneumonic phthisis, cheesy pneumonia, tubercular phthisis, and fibroid phthisis, are all manifestations of the one disease. Such a classification may be, however, entirely justifiable and useful for practical clinical and therapeutic purposes. Pathologically considered, phthisis is a local tuberculous inflammation of the lung which may manifest itself in various ways, the appearances depending upon the duration of the disease, the mode of onset, and the constitution and condition of the patient. Lesions representing the different forms of phthisis, and their

* Cancer and tubercle are considered analogous lesions, and classed with tumors, by a number of pathologists. This fact would not make it inconsistent to call tubercle an inflammatory product, as the distinction between inflammatory processes and tumor formation is a purely arbitrary one. Virchow pointed out that the majority of tumors are purely inflammatory products (a statement antedated twenty years by Prof. S. D. Gross). A few years ago I made the question of the etiology of tumors a subject of careful personal study, which I yet continue, and I am forced to the conclusion that *all* true tumors are inflammatory products, and that no line of distinction can be drawn where the process which we call inflammation ends and where tumor formation begins.

transition from one form to the other, are often seen in the same lung.

Virchow insists that nothing should be considered tubercular unless it shows true tubercle nodules, and hence he does not recognize cheesy pneumonia—or cheesy hepatization, as he calls it—as tubercular, although he does not object to the term phthisis for this lesion.

I was fortunate enough to attend several times the classical demonstrations of Virchow on this point, the father of the view of the dual origin of cheesy matter and phthisis; yet, from our present knowledge of what constitutes tubercle, I cannot help interpreting all the forms of phthisis as of a unitarian origin. It is, after all, as Virchow himself says, only a matter of nomenclature. If we consider the presence of bacilli of Koch as the differentiating point between what is tubercular and what is not, we find that catarrhal and cheesy pneumonias are the most tubercular of all, because they contain, as a rule, more bacilli than any other forms of phthisis.

Although cheesy pneumonia, like all forms of phthisis, remains commonly a local affection, it is seen on the autopsy table to give rise to miliary tuberculosis at least as often as any of the other forms of local tuberculosis.

We are then at present at the same standpoint in regard to the character of tubercle and cheesy matter as Lænnec (1819); and it is indeed perfectly reasonable to suppose that any cheesy matter found in a scrofulous person or animal is tubercular. Of course it is evident that tuberculosis of the lung is usually accompanied by simple inflammatory products, such as organized connective tissue (chronic phthisis), or unorganized croupous and catarrhal exudates (predominating in acute phthisis), which may undergo rapid necrotic and purulent changes, resembling cheesy material. For the latter products the name “coagulation necrosis,” as applied by the Heidelberg and Leipsic people, may be employed. Tubercle bacilli are commonly found in this coagulation necrosis. True tubercular cheesy matter should, I think, be considered only that product which is derived from the breaking down of previously well-organized tubercle tissue.

I need not refer to the details of the manifestation of tubercle in the lung, as these are too well known. But I would like to remark here that those small whitish or gray nodules, usually of

somewhat irregular shape, which are seen more or less densely scattered throughout the parenchyma of lungs affected by phthisis, are not miliary tubercles, but minute foci of broncho-pneumonia.*

These miliary broncho-pneumonic foci take their origin from tuberculous matter disseminated by means of air-passages, as explained before. Miliary tuberculosis of the lung distributes itself by means of the perivascular lymphatics, is very rarely accompanied by catarrhal changes or hepatization, and rarely arises from a primary tuberculous focus of the lung itself; it is, as a rule, a part of general tubercular disease.

II.—THE PREDISPOSITION.

Having shown that for the production of tuberculosis we need a special soil, and that the irritant is only of secondary significance, some inquiry into the nature of this soil is necessary.

The question of the predisposition to tuberculosis, as it stands at present, must be considered from three aspects:—

1. The clinical aspect.
2. The anatomical aspect.
3. The bacteridian or parasitic aspect.

The consideration of the clinical aspect of the predisposition to tuberculosis is invaluable, as it rests mainly on actual observation, on demonstrated clinical facts, and on conclusions drawn from statistics.

From time immemorial, a clinically well-defined condition of the system, known as the strumous diathesis in its various forms, has been recognized. This condition will be considered later on.

There are a number of ailments which, from the experience of clinicians, are known to have a great, direct or indirect, influence in the development of general tuberculosis and pulmonary phthisis; or are known to create conditions of the system that predispose it to this malady. Such are syphilis, inflammation of serous membranes, bronchitis, croupous pneumonia, diabetes, the exanthemata, especially measles and typhoid fever, deformities of the skeleton, rickets, cerebral and spinal diseases of various kinds, dyspepsia, the puerperal state, uterine diseases, prolonged

* See, in connection with this, the excellent studies of Wm. H. Mercur, from the pathological laboratory of the University of Pennsylvania, published only in abstract form in the *Phila. Med. Times*, July, 1888.

nursing of children, Onanism, change of climate, continuous loss of sleep, distress, etc.

That exhaustion, exposure, the deprivation of food, and other hardships of campaign life, etc., are prominent etiological factors in the production of pulmonary consumption is learned from the accounts of military surgeons, who observed among young, robust soldiers a remarkable increase in the morbidity and the mortality of phthisis, during and immediately after the close of a war. Such observations have been made in the Franco-Prussian and Turko-Russian campaigns. The fact that consumptive soldiers are not allowed to enter upon a campaign (certainly not in Germany and Russia) excludes here the probability of contagion.

Statistics also show the remarkable prevalence of phthisis in persons of certain occupations, such as stone-cutters, miners, cigar-makers, weavers, telegraph operators, book-keepers, and persons engaged in certain other occupations of a more or less sedentary nature. It is more natural to suppose that the disease or the predisposition to it is created by the character and the conditions of the occupation, than that a contagion should affect preferably shoemakers, miners, or soldiers in the battlefield. Again, in most phthisical patients the beginning of the disease can plainly be attributed to an exposure, to "a cold."

On the other hand, there are pathological conditions or diseases which appear to prevent the development of phthisis and tuberculosis in general. It is an established clinical fact that phthisis is extremely rarely, if ever, associated with mitral heart-disease; and, from my own observations, I believe that phthisis is rarely coincident with tumors. For the latter circumstance, I can offer no explanation; nor is there any statement to this effect in literature. Rindfleisch has suggested that heart-disease prevents the development of phthisis by inducing repeated slow congestions of the lungs, these congestions producing an overgrowth of the muscular tissue of the bronchioles and air-vesicles, which thus gains strength for repelling the exudates following inflammation.

If tuberculosis were depending upon a contagium for its development, neither heart- nor tumor-disease, nor any condition of the organism, could ever prevent its occurrence.

All the clinical facts above referred to prove definitely the

necessity for a predisposition for the development of tubercular disease, and militate against the necessity of a contagium.

The anatomical aspect of the question—the morphology of the soil in which tubercle develops—is the most important aspect.

Beneke * tries to explain the disposition to tuberculosis by a disproportion between the size of the heart and blood-vessels and other organs to the bulk of the body.

Schottelius † made recently some interesting observations concerning the mode of termination of the smallest bronchioles and their relation to the lung acini in different animals. He found that in the carnivora the entrance of the bronchioles into the acini presented very small apertures, so that the air-vesicles were not easily accessible to irritants; while in the herbivora the terminal bronchial terminations were quite wide, thus permitting the free entrance of irritants. He states that in man the bronchial terminations congenitally approach sometimes those of the carnivora, and sometimes those of the herbivora. In the latter type, he believes to have found an anatomical explanation for the predisposition in some individuals to pulmonary tuberculosis. ‡ Weigert, of Leipsic (one of the most enthusiastic germ-theorists), properly remarks upon the observation of Schottelius, that it does not explain the predisposition, as the same animals will react, upon the introduction of the “poison of tuberculosis” into any other part of the body, where the bronchials do not come into play.

My own studies upon the minute anatomy of the tissues of man and of animals predisposed to tuberculosis, extended over a large amount of material, and gave results which, to my mind, satisfactorily explained this condition. These results I announced at a meeting of this Society in October, 1882.

The anatomical peculiarity observed in either man or animals, be it inherited or acquired, I first showed to be, briefly stated, as follows: all the tissues of the body approach somewhat an embryonal type—they are peculiarly rich in nuclei and young cells, and the lymph-spaces of the connective tissues are narrower,

* Die erste Ueberwinterung in Norderney, Norden, 1882.

† Virchow's Archive, vol. xci, 1883.

‡ The method of investigating this condition is not without interest. The vesicular structure of the lung was injected, through the bronchi, with a resinous melted mass, which, on cooling, presented molds of the bronchials, in connection with their characteristic infundibula and acini.

fewer in number, and show a great many more cellular elements in the scrofulous than in the non-scrofulous. So far, subsequent observations of others agree with mine. Objections are raised only as to the direct relation between these structural peculiarities and tuberculosis. Here I must state that I only suggested, and never asserted, the necessity of such a relation. It is quite possible that there are some other and more striking peculiarities in the morphology of scrofulous animals yet undiscovered. This much, I can, however, reassert to-day: that tuberculosis promptly ensues when a simple inflammation is set up by any kind of injury, in animals with the structural peculiarity which I have described; but tuberculosis is not produced in animals that do not have this structural peculiarity, so far as my experiments show, unless the injury is inflicted upon serous membranes.

For the details of my researches in this direction, I must refer to my first paper upon this subject.*

Koch asserts that the structural peculiarities of the tissues which I described can have no etiological relation to tuberculosis, because an animal not possessed of such tissue peculiarity—the cat—is easily inoculable. Here I must differ from Koch, as in my experience with cats this is not the case; and, again, Koch brings no proof for his assertion, and I am unaware that he, or anybody else, produced tuberculosis in a cat, except by inoculation into some serous cavity. That inoculations into serous membranes prove nothing for tuberculosis, as I have shown conclusively, Koch still seems to fail to see. But here is a way in which cats may become tuberculous, with or without the bacillus. In one instance, we kept one of the cats in a close box, *deprived* of liberty, good air, the comforts of life, motion and sufficient food; she also had been inoculated with diphtheritic material eight months previously, but had recovered. After the lapse of a year, the cat was set free; but was accidentally killed, and was found to be affected by general tuberculosis in a high degree.

This, in my opinion, corresponds fully to the conditions in which a healthy young woman is placed, and finally becomes scrofulous, and then tuberculous, from a simple cold, after being the faithful nurse for a couple of years of a consumptive husband.

* *Loc. cit.*

On the other hand, there is full reason to believe, as it is in accordance with experience, that young scrofulous persons, under proper conditions, may become normal individuals; *i. e.*, lose or outgrow the predisposition to tuberculosis. (I have dwelled upon this in my first communication on this subject.)

The scrofulous habit, and consequently also phthisis, may skip a generation and does not invariably embrace all members of a family. It has been observed that parents may have at first healthy children without any vice, who grow old well; and subsequently the same parents, without being phthisical (but perhaps otherwise becoming deficient in health), may have other children that exhibit a full scrofulous habit. But even the reverse has been observed.

It would be highly desirable if physiologists would furnish some experimental observations on the circulation of the plasma in the lymph-spaces. This is, to my mind, a circulation or movement of vital juices in the tissues, which, for the well-to-do of the individual, is of importance next to that of the blood. These important channels, the lymph-spaces, are known to regulate the blood-pressure, carry and breed (white blood corpuscles) food for the tissues, lubricate tissues and relieve the body, if any of its parts are damaged by injury of any character, of inflammatory exudates, dropsy, etc. These channels are nearly blocked up, nearly useless in the *scrofulous*, and hence cannot perform their functions; and thus modify materially the condition and the fate of the individual, in case of disease.

The term "*scrofulous*," which I retained for describing the above-stated anatomical peculiarity of animals and individuals, is as good as any other term; moreover it is known by all as designating the "predisposition" to tuberculosis. Scrofulosis should be called a *condition* and not a disease, as it has its (a natural) hereditary and widely distributed type in man, and its homologue in some normal animals (rabbit, guinea-pig, etc.). It must be remembered that the scrofulous individual acquires certain lesions, such as enlargements of lymphatic glands, cold abscesses, caries, long-standing catarrhs of various kinds, skin eruption, and certain deformities of bones, only under the influence of injuries, or of the same agencies which, in the non-scrofulous individual, lead to transient and curable affections.

Virchow designates simple, permanent enlargement (hyper-

plasia) of lymphatic glands, with or without cheesy change, "scrofulous," in contradistinction to "tuberculous" lymphatic glands, which contain miliary tubercle nodes (heteroplasia), and which also undergo cheesy change.

There is nothing called "scrofulous" or "scrofulosis," which is not also called "tubercle" or "tuberculosis" by others. There are, strictly speaking, no scrofulous products, but only tuberculous products. The traditional term "scrofulosis" is variously used and interpreted, although it is not evident that any one means by it anything anatomically well defined.

Others take matters easier, calling everything *tuberculous* that contains tubercle bacilli, and calling *scrofulous* all cheesy matters in which bacilli are absent.

There is still a third aspect of this question, viz., the parasitic or bacillary theory of the predisposition to tuberculosis. As I mentioned in the earlier part of this paper, Baumgarten and several others recently brought forward that the predisposition to tuberculosis is to be explained by the susceptibility of an individual to bacilli! Under this hypothesis, the scrofulous tendency in individuals is created through the mediation of the bacilli. It is supposed that the bacilli or their spores may be conveyed to the ovum by the organism of the mother, or in utero by the spermatozoa of the father. Furthermore, they say, inheritance is to be explained in no other way than by a bacillary infection of the infant through the milk of the nursing mother, and by subsequent living together of children and phthisical parents.

We may exclude such view altogether from consideration, as it has not been proven. Besides, it is not in accordance with facts from observation. It is as contrary to biological laws to accuse parasites for the transmission of a predisposition to tuberculosis, as it would be for that of epilepsy, etc. Hence we may dispose of such view as an unfounded, absurd hypothesis.

I am not opposed to the germ-theory of disease, where it has its well-founded and proper application. Bacteridian studies have contributed largely to our knowledge of a certain class of pathological processes and lesions. But misinterpretations of the significance of bacteria; bacillary speculations, without occasion for them and without any proper application to the subject, are a check to the progress of medical science. The question of the predisposition to, and the cause of, tuberculosis, demands a great

deal more of solid pathologico-anatomical and experimental studies; it can, by no means, be regarded as settled, and least of all through the discovery of a bacillus inhabiting necrotic tubercular tissues.

III.—TUBERCULOSIS, WITHOUT PREDISPOSITIONS, DUE TO INFLAMMATION OF SEROUS MEMBRANES.

For some years I felt much interested in the question whether or not simple inflammation of serous membranes could lead to tuberculosis in the non-scrofulous, that is, in persons which have no family history of tubercular disease; and I would like to ask the opinion and experience of the members of the Society upon this question. It is well known that there may be primary tuberculosis of serous membranes, producing secondary inflammations; and, on the other hand, tuberculosis secondary to adhesive pleurisy or peritonitis is also common in serous membranes. The general belief, however, is that whenever tubercular disease in either case occurs, if not secondary to phthisis or tubercular disease elsewhere, a strumous or scrofulous condition is required.

Traumatic injuries of joints are known to lead often to fungoid (tubercular) synovitis and general tuberculosis occasionally in individuals with good family history. Simple injuries of the eyeball (the anterior chamber as well as joints is lined by serous membranes), under conditions as above stated, have also been known to lead to tuberculosis, as recorded by Wolfe (*British Med. Journal*, March, 1882); Gradenigo (*Annale d'Oculistique*, 1870).

Dr. M. Litten,* of Berlin, was the first to publish some accounts which demonstrate that miliary tuberculosis may be caused directly and primarily by pleurisy and inflammation of other serous membranes in persons with no phthisical history, and without any cheesy masses being formed in any part of the body. In his (Litten's) experience this was particularly the case when there was a rapid reabsorption of the exudates in case of chronic pleurisy, or if repeated removal of the fluid of a hydrothorax or ascites by tapping has been performed. He records several well studied cases of that kind, accompanied by autopsy records.

* M. Litten, Sammlung Klin. Vorträge, No. 119. Ueber acute Miliartuberculose, 1877. For further references see Wiener Med. Presse, No. 36, 1882; Charité Annalen, vol. vii, Berlin; Krankheiten der Respirations-Organen, in Virchow's Handb. der Spec. Path. und Ther., vol. i; Virchow, Geschwulste, vol. ii, p. 725, etc.; also, Formad, Transactions of the Phila. County Med. Society, and of the Pathological Society, for 1882-83.

Litten's observations at no time, however, received their well deserved attention.

Not only clinically; but also pathologically this part of the tuberculosis question is rather neglected. In text-books of pathology the occurrence of primary tubercle in adhesive bands is incidentally mentioned; but no special consideration is devoted to its etiology and manifestations.

Upon the autopsy table I have repeatedly met with subjects with exquisite primary tubercular peritonitis, pleurisy or pericarditis, which, upon inquiry into the history of the cases, failed to reveal any phthisical or scrofulous history. The products of these inflammations were often plastic in character, not unlike those of fungoid synovitis. The appearances sometimes present themselves particularly strikingly in the peritoneum; all the viscera may be glued together by plastic material into a solid mass. The omentum is usually retracted and matted together into a solid cord or mass, which, laying parallel with the transverse colon, reaches across the abdominal cavity, and may have a thickness of from two to four inches; the mesenteric and other lymphatic glands are usually normal, but sometimes in advanced cases may be much enlarged and more or less cheesy. The perfect absence of any cheesy focus in the body is, however, often a conspicuous feature in these cases.

Some pathologists deny the tubercular nature of these formations and of the flat nodular masses which cover the serous surfaces in these cases. It is true that fibroid changes predominate in these formations; but numerous tubercle nodules, with all the necessary attributes, epithelioid and giant cells, and necrotic changes, were plainly seen in all cases which I had occasion to examine. Secondary miliary tubercles of quite recent date are also found thickly strewn locally in these parts, and may or may not be seen in the lungs and other organs. As a rule, there is more or less ascites in these cases. My colleague, Dr. E. O. Shakespeare, has recorded similar cases, and Dr. Morris Longstreth tells me also that he had seen and studied such cases. Dr. Mitchell Prudden describes (*New York Med. Record*, June 16, 1883) an allied case.

In chronic adhesive pleurisy there occur similar primary tubercular formations in the organized plastic exudate which in some cases gives rise to secondary (miliary) tuberculosis of other

organs. The lungs may be perfectly normal in all parts, and show only peripherally, just below or bordering the pleura, some indurations of gray color made up of recent tubercle tissue. These young tubercle infiltrations are in some cases seen to have penetrated into the substance of the lung, like in a pleuro- or dissecting-pneumonia, directly from the old tubercular masses of the adjacent pleural membrane.

I have also examined several cases of plastic adhesive pericarditis, and found the plastic vegetations in this lesion to contain tubercles; two of these had coincident pleuritic lesions.

Cases which came under my observation during the last eighteen months—*i. e.*, since the opening of the bacillary campaign—were, of course, carefully examined for bacilli, and the results may be summarized as follows: bacilli were found in most of the lesions, if the tubercular disease of serous membranes was accompanied by cavities and cheesy masses in the lung, or by tubercular ulceration of the intestines, and if cheesy changes in general were prominent; but no bacilli could be discovered, even after repeated and careful search, in any of the lesions of four cases of primary peritoneal and pleuritic tuberculosis examined. In none of these latter four cases were there any conspicuous cheesy changes in any organ, and no cavities or marked hepatizations in the lung, and no intestinal ulcers, although in two there was slight pulmonary miliary tuberculosis. These cases will be recorded in detail in a future publication.

I have also seen several cases of primary tubercular pleurisy and pericarditis, and a few of primary tubercular peritonitis, in the pathological institutes of Virchow in Berlin, and of von Recklinghausen in Strassburg. I questioned these foremost men of pathology concerning the etiology of these lesions. They, as well as Rindfleisch, of Würzburg, told me personally their opinion, stating their firm belief that these lesions often directly originated from simple chronic inflammatory changes, without the agency of any cheesy focus, or any specific agencies whatsoever.

Birch-Hirschfeld also states, in his classical pathological work (page 183), that "nearly every exudative pericarditis and pleurisy leads to a local tuberculosis, if it takes a chronic course."

How often primary tubercular lesions of serous membranes occur in non-scrofulous persons, and whether this is the only

form of tuberculosis in this class of persons, is, of course, a matter of speculation, until thorough statistics and careful studies are made in this direction. Nevertheless, it is a demonstrated fact, as I will show further on, that primary tuberculosis can be produced in the peritoneum of animals, like the dog, which are proved not to have any scrofulous tendency. I have seen this myself, and seen others succeed in this experiment, by the introduction of simple irritants into the peritoneal cavity. Koch also never succeeded, even with the bacillus, in producing tuberculosis in the dog, except when using the peritoneal cavity or the anterior chamber of the eye (which is also a serous sac) as a point for inoculation.

Here is room for hypothesis. I would prefer to believe that tuberculosis could occur only in scrofulous persons, as this would better agree with the *scrofulous anatomy*. It is, however, possible that a scrofulous anatomy of the tissues may be artificially established by the blocking up of the lymph-spaces of the serous membranes, by fibrine and molecular *débris*, suspended in the serum which is being reabsorbed. This would then be a mechanical process, and not one of infection. If an inflammation occur in serous membranes, resolution becomes difficult through the peculiarity of the exudate. This is fibrinous mainly, and forming extensive, usually permanent organized deposits, it impairs the function of serous surfaces quite materially; the reabsorption of new exudates is probably entirely impossible. Thus conditions may possibly be created in serous membranes, not unlike those of scrofulous tissues, and simple irritants, perhaps the fibrine, may induce in them a similar reaction.

IV.—QUESTION OF CONTAGIOUSNESS. CLINICAL ASPECTS.

The idea of the contagiousness of tuberculosis is not new and, like other unfounded views in medicine, it has oscillated; like all fashions will, from one extreme to another for many generations. At present it is entertained by a number of scientists and by a part of the profession.

This view has called forth, from time to time, a number of researches whose results were either pro or contra. I will refer to these subsequently.

Of late, it appears that the belief in the contagiousness of tuberculosis has won considerable ground, not so much on account

of accurate observation, as on account of Koch's discovery of the bacillus tuberculosis.

Another element, which seems to have had an influence in this direction, is the fact that certain experimenters, formerly believing, from their own experiments, that tuberculosis was non-contagious, were led, later on, to change their opinions on account of the results of subsequent experiments. These latter experiments will, however, be shown to have been imperfect.

Before discussing the merits of the bacillus question, I would like first to consider the question of contagiousness from clinical grounds; and should it be proven that tuberculosis is not contagious, then the necessity for a contagium surely falls to the ground.

According to the observations of the most prominent clinicians who have paid special attention to this matter, there is not a single authenticated case of tuberculosis as a result of contagium on record. Among scores of experienced men who deny thus the contagiousness of phthisis it is sufficient to mention the names of Paget, Humphrey, Richardson, of London; Bennet, in France; and Hiram Corson, in our own midst—all men of close observation, with ripe experiences reaching over fifty years.

The statistics of the large Brompton Hospital for consumptives, for thirty-six years, with regard to the resident officials, compiled by Dr. F. Williams (quoted after the *Lancet* —), shows that of four resident medical officers, one of whom had served twenty-five years, none had any lung disease; of six matrons, none were consumptive; of 150 resident clinical assistants, eight became consumptive and five died, but in only one was the disease developed during residence at the hospital. Since 1867, of 101 nurses, only one died from phthisis, and that after leaving the hospital. Before 1867, six died, three of these of phthisis, but only one became so whilst resident, and she had a consumptive sister. She died thirteen years after first joining the hospital, but was not there the whole time. Of thirty-two gallery maids since 1867, none developed phthisis whilst at the hospital. Of twenty house-porters, five died, but none of consumption. Non-residents:—Of nine secretaries, three were threatened with lung disease, but recovered. Of twenty-two dispensers, seven died, three of phthisis, one while at the hospital. Of four chaplains, three died, none of phthisis. Of twenty-nine physicians and assistant physicians,

eight died, none of phthisis. At the Chest Hospital, Victoria Park, there have been five resident medical officers during about the last fifteen years; all are alive and well. Two matrons, neither consumptive. There were two clinical assistants appointed every three months; none known to have developed the disease at the hospital. One nurse out of fifty or sixty in the last few years became consumptive while at the hospital, and she died after a year's illness.

An ingenious plan to decide the question of the communicability of phthisis was instituted by the British Medical Association by establishing the Collective Investigating Committee. This committee sent out questions relating to this subject to all the members of the Society. Of 1028 replies received, 673 negatived the idea of a contagium, while 261 replies favored it. According to these statistics, there is a manifest majority in favor of the non-contagiousness of phthisis; yet such a plan is unsatisfactory, as the answers may be of unequal value, as their worth must be estimated in proportion to the experience and authority of the sender.

Not without interest is the observation of Prof. Corradi, of Pavia, who noted that out of 133 families in which he had cases of consumptives, in only twenty-five of the families were there more than one member of the family ill of that affection.

There is no proof whatever that tuberculosis is conveyed from person to person by contagion. Seeming exceptions to this assertion can almost always be accounted for in some other way.

The assertion that the wife may contract the disease from the husband, I have pointed out, in a former paper, to be untenable; and I have also indicated that a predisposition to scrofulosis may be acquired from the unwholesome mode of life led, of necessity, by such individuals. Besides, it is established statistically that nearly one-third of all deaths occurring in middle life are due to phthisis. In view of the frequency with which this malady occurs, intermarriage between scrofulous individuals may be almost as common as between non-scrofulous ones.

The view taken that children become scrofulous by contagion from phthisical parents, may be met by the fact that instances have occurred where a number of young children of phthisical parents were early removed from their homes and distributed

among healthy families, and yet all, sooner or later, became phthisical.

Healthy persons have even been fed on bovine tuberculous material (which is considered identical with human tuberculous material) and have thrived on it, as is proven by the interesting feeding experiments made upon man and recorded by Schottelius (Virchow's Archives, No. 91, 1883). The circumstances which led to this experiment were as follows: In Würzburg, the sale of meat affected by pearl-disease or bovine tuberculosis is permitted, but, as some opposition to its sale once arose, a community of country people agreed to use exclusively tuberculous meat, on account of its cheapness and in order to prove that it was harmless. From October, 1867, to November, 1868, forty-nine tuberculous beeves, with well-pronounced lesions, were consumed by these people while they were under the supervision of the district physicians. In many instances the meat was even eaten raw in consequence of habit. Ever since then, those people have continued the use of tuberculous meat, and thus far no bad results have been noticed; in fact, the record says that the people referred to are unusually healthy.

[TO BE CONTINUED.]

DISCUSSION ON CONTAGIOUSNESS OF PHTHISIS.

DR. J. SOLIS COHEN, in opening the discussion, said: I can only say that it requires a great deal more proof than has yet been offered, to convince me of the contagiousness of tuberculosis, in the ordinary sense in which the term is used. If tuberculosis be contagious, it is certainly so very slightly only. Some fifteen months ago, in the discussion on Dr. Formad's first paper, I mentioned one or two marked cases of communicated phthisis which had occurred in my practice, such cases as we have all seen. It occurred to me that on the present occasion I might make a personal allusion to myself, as having for a great many years been particularly exposed to contagion from tuberculosis. I have sat day after day in front of patients with tuberculosis, and these the worst kind of cases, with tuberculosis of larynx and lungs. I have washed out their larynges and have not only inhaled their breaths, but sometimes, accidentally, their sputa; and, a few years ago, I even acquired a cough which gave me some concern for several months, and yet I have escaped contagion. In all large medical centres there are so many men extensively engaged in examining patients far gone in tuberculosis, that, if it were contagious, some of them should have succumbed from this cause. I have little of importance to say in regard to the presence of the bacillus; I have had a great many cases examined for me, among them several cases of undoubted tuberculos's, in

which we have been unable to see the bacillus. Perhaps this was because we were not familiar enough with the necessary manipulations. In a case of undoubted tuberculosis which we examined two or three weeks ago, a case which has attracted some attention on account of the plan of treatment pursued, there were bacteria in the sputum which resembled tubercle bacilli; but they would not retain any of the stains. They lost all their color upon the addition of nitric acid.

Dr. Prudden, of New York, already alluded to by Dr. Formad in his paper, who examined a number of cases of tuberculosis, was unable to distinguish the bacillus in any of the several tuberculous organs in a case of general tuberculosis. While he does not say that it was not there, he states that he could not discover it. I am sorry to see, from reports published in the journals, that the opinion that tuberculosis is a contagious disease is gaining ground, as I can easily understand how it is going to bring sorrow to many families.

There is another point to which I wish to allude, and that is the disappearance of this bacillus. It is becoming more and more common every year for patients to recover from tuberculosis, and as they get well the bacillus disappears. These cases go far to prove a remark made by Dr. Formad, that it requires a proper soil in which to be developed. I can hardly think that all cases of tuberculosis are due to exposure to the bacillus. If so, then we must believe that as we improve the nutrition of our patients, the bacillus becomes no longer capable of propagation. I cannot help believing that the presence of the bacillus is more a result than a cause, and that the reason we find it is because tuberculosis has already been established.

DR. BRUEN: I rise to speak to the point of the contagiousness of phthisis, not because I think the question at this time can be thoroughly settled, but because I think it important that all who have had the opportunity of witnessing the progress of this disease, should put on record their experience. I think it possible that the mistake in reference to the contagious character of phthisis has arisen from the apparent impossibility of ignoring the agency of the bacillus in the etiology of certain cases of phthisis. As Dr. Formad has shown, the bacillus can be recognized as one among a number of substances which, after being introduced into the air inspired, or into the circulation, can produce changes in the lungs which we regard as tuberculosis. He has also shown that there are certain differences between the character of the lesions produced in the lungs in animals and in men. The fact is well established, that there are many organic and inorganic substances, which, if introduced into the lungs, produce tuberculous changes. As one writer has put it, if one hundred individuals in different parts of the world, whose reliability cannot be questioned, have seen geese flying, and certain others have not, the testimony of the latter will not weigh much against that of the former. Contagious diseases are usually traceable to a single cause, but quite a number of different influences give rise to phthisis.

From a clinical point of view, it seems to me we must all have a certain number of cases in view, in which the possibilities of environment suggested

contagion, but if phthisis was in any way contagious, it would not be necessary to minutely examine the records, as we would have any number of instances of the contagion, just as we have in small-pox or scarlet fever.

I have seen families where one or two members have died and left behind them a predisposition by inheritance to phthisis, which was augmented in their offspring by the build of the chest, and yet they have escaped entirely. I think that the fact that those who have spoken recently in discussions in reference to this matter, have been obliged to ransack their case-books to find instances of contagion, militates against this view of the etiology of phthisis. There is another point against it. We all know of cases of phthisis, which, when once placed under favorable hygienic conditions, as in the Adirondacks, California, or elsewhere, often improve and practically recover; and when these cases return to such an atmosphere as we have here in Philadelphia, the pulmonary process is again lighted up, but it does not pursue the course of a parasitic disease, but of an inflammatory one. They take cold, a catarrhal form of bronchitis is observed, and the whole course is inflammatory rather than parasitic. These are facts which we cannot set aside, and it occurs to me more and more, as my own experience testifies against the contagion of phthisis, that a partial truth has been magnified into a law.

DR. TYSON: It appears to me that we can best arrive at the status of the subject by a series of questions, some of which may be among those propounded by Dr. Formad. The first is: May the bacillus tuberculosis be considered a constant anatomical element of true tubercle? That it is very often such an element is now generally admitted, but there are still numerous observations to show that it is wanting at times. Yet it seems to me, in watching the matter from a disinterested standpoint, that, as the experience of practical histologists increases, they are more and more successful in finding it. Reasoning from this fact it would seem not unreasonable to expect that ultimately it will be found forming a histological element of all tubercle. Still the studies of many skilled histologists show that it is occasionally wanting. The observation of Dr. Formad, which makes it invariably present in degenerated tubercle, is an important one.

Secondly, is the bacillus capable of producing tuberculosis if inoculated? Even Dr. Formad admits that it is one of the substances capable of producing tuberculosis. But this answer immediately suggests the third question. Is any other substance capable of producing true tuberculosis by inoculation? and here, it is evident, the whole question hinges. If it can be proved that other things, as ground glass, inspissated pus, fragments of cheese, etc., can do this, then, of course, the idea of the infectiousness and contagiousness falls. It must be admitted that the number of those who believe that this result can be produced by other substances is diminishing. The name whose withdrawal from the view that tuberculosis is producable by the inoculation of indifferent substances withdrew most support from it is that of Cohnheim, whose original experiments favored it. And, now most recent, is that of Dr. Wilson Fox, one of the best known of English experimenters. There still remain, however, quite a number of authoritative

names besides that of Dr. Formad, who claim that tuberculosis may be thus produced. Such are Burdon Sanderson, in conjunction with whom Fox's original experiments were made, who, so far as I now know, has not yet changed his views, Empis, Papillon, Nicol and Levhan among French experimenters, Lebert, Wyss, Waldenburg and Guttman among the Germans, and Robinson in America. It must be admitted that these are offset by a large number of modern and skilled investigators, but the number on the other side is still too great to permit them to be ignored. Until a greater unity of result in experimentation can be arrived at, the question must remain *sub judice*.

The clinical study of tuberculosis is altogether against the contagiousness of the disease. No one of experience to-day denies that tuberculosis of the lungs most frequently, if not always, starts in a bronchitis, the result of exposure to cold. Does any one claim that small-pox, typhoid fever, scarlet fever or any other infectious disease originates in this way?

On the other hand, facts are accumulating rapidly, which go to show that the bacillus tuberculosis is of diagnostic value, that when it is found in the sputum there is good reason to believe that tuberculosis is present. But this has nothing to do with the other question as to its infectiousness.

In conclusion, I may say I cannot as yet bring myself to believe in the infectiousness of tuberculosis, although I admit that some experimental results favor such a view; but that the question cannot be considered settled so long as such diverse results occur in the experiments instituted for its solution.

DR. MORRIS LONGSTRETH: I have no disposition to speak on this debate, as I disagree so entirely with those who do most of the writing on this subject at the present time, not only in regard to tuberculosis, but also to the so-called germ theory of disease.

We have been too much given up to the charm of the word *germ*. We ought never to have yielded to this influence which it has acquired over our senses, and under its spell we have somewhat lost our bearings. The word found acceptance about the year 1850, and was at first used in a purely imaginative sense, to express the material qualities of contagious or infective matter, as distinguished from virus and miasm, those unknown and imperceptible agents which had hitherto been considered the agents by which disease was propagated.

Following this in time came the discussion of the question of spontaneous generation, and out of this arose the belief—some would call it absolute proof—that all processes of fermentation and decomposition were due to and conducted by minute organism. Some, I believe, would even go so far as to assert that healthy functions within our organisms are carried on in the same manner, by the same agents.

From this mass of facts our present germ theory seemed to be a natural outgrowth. The question, in every respect, seemed to some so straightforward, easy and conclusive. We had the seed of a fungus; it became lodged in our tissues, finding access by various ways; it grew like a tree—and verily some speak of disease as though it were a growth within the

organism, instead of looking on disease as a perversion of function. Or others have viewed the whole matter as a fermentation. And truly the analogies between the two processes—putrefaction and disease—when handled by a skilful pen, afford the opportunity of making many beautiful and poetic metaphors; but all this does not help us much to understand disease, and it furnishes no proof that disease is caused by micro-organisms.

We have yielded to the charm and to the evidence or half-evidence of something else, without receiving any proof distinctively that micro-organisms have the inherent property of perverting our bodily functions. The argument stands thus: Organisms are present, therefore they do it. We are without analogies that they are capable of thus acting in nature. The comparison has been made with the parasitic entozoa and ectozoa; but one cannot be certain of an intestinal worm unless one is passed per anum, and many of skin parasites make but little disturbance, unless irritated by the finger-nails. In most respects there is no sameness in the effects, whatever we may think of the comparison. *Filaria* are said to produce a definite form of disease in the East Indies, but *filaria* have been known to swarm in the blood of men in apparent health without producing symptoms.

As to the question of the particular bacillus of tuberculosis, it has been stated that we are not able to recognize tubercle except through the presence of the bacillus. This criterion is certainly very unfair, and is practically begging the whole question in the argument. A competent pathologist, having before him the history of a case, and a personal knowledge of the symptoms, on making the post-mortem examination and examining the tissues microscopically, ought not to fail to distinguish the presence or absence of miliary tubercle. Caseous masses in the lung, of which there are so many varieties as to their origin, look very much alike, and as much like a piece of cheese from the grocer's as anything else. These caseous masses seem to be the stumbling-block of so many observers; but there is not any use attempting to decipher the origin and nature, whether tubercular or not, of a thoroughly caseated mass, which no longer has any structural elements. It cannot be done, and it is time we acknowledged it.

I claim that we ought, under fairly favorable circumstances, as already set forth, to be able to recognize a structure in the lung or other tissues which is distinctly a miliary tubercle. Perhaps we may not be able to define it in words, but that does not make it any less distinct and definite in its characters. A competent pathologist would not mistake it for a young syphilitic gumma, or perhaps three or four more little nodules, which one, who has served his time properly in the hospital wards and dead-house, knows do occur in the tissues. Many of those observers abroad, who are doing most to lead public opinion at the present time, have not had this opportunity of learning their proper lesson, if we may judge by their writings. Too many of these observers have qualified themselves merely in methods of experimentation, leaving out of account the methods of nature. Experiments, like statistics, can be made to prove almost anything.

In examining tissues, which, according to the ordinary acceptation, are

tubercular, I have found them without bacilli. The absence of bacilli in one case is sufficient to knock down the whole theory.

The following case illustrates the point: A young man (with a family history and personal antecedent entirely free from phthisis), after an ordinary gonorrhœa, followed by a cystitis, was seized after the lapse of considerable time with a congestion of the lungs, apparently threatening a double pneumonia. Death resulted very promptly, and at the post-mortem examination it was found that the left kidney was filled with large blocks of cheesy matter. The ureter was occluded by the same sort of material; at its entrance into the bladder was an enlarged lymphatic gland which had pressed upon it and closed the communication with the bladder. The latter viscus showed the results of a recent cystitis, but was free from ulceration. The chains of lymphatic glands leading from the kidney were enlarged very much, but not cheesy. The cause of congestion of the lung was found to be due to innumerable miliary tubercles; they seemed scattered through its tissue like minute grains of sand; no solidification, no cheesy areas were present. Several other organs showed miliary tubercles, but the bladder, although slightly ulcerated, was healing and had no tubercles in or around its walls. It was not a case of primary tubercular inflammation of this organ.

I wish also to detail a second case, in which a man, of about fifty years, after an injury to the right inguinal region, with evidences of subsequent local inflammation, died, having lung symptoms and severe diarrhœa. The autopsy showed an abscess with inspissated cheesy contents near the head of the colon. The lungs and other organs showed numerous miliary tubercles; the peritoneum was also affected with a similar deposit, and the connective tissue behind the peritoneum and around the seat of the abscess was found especially full of tubercle.

Such cases do not need very much comment, and they seem to show pretty conclusively that the body is capable of elaborating material within its own tissues that leads to the deposit of tubercle—to show, too, that a foreign entity (micro-organism) has, in all probability, nothing whatsoever to do with the question.

In response to written requests made by the President of the Society, the following communications were received and read:

DR. TRAILL GREEN, of Easton, Pa.: In a practice now reaching almost fifty years, I am very free to say that I have never seen a case of phthisis which I had the slightest reason to believe was traceable from a wife to her husband, or from a husband to his wife. I have known life to be extended through more than forty years after the death of a partner in the married state. In this long practice I have seen cases in both sexes, and survivors, male and female, of various temperaments and degrees of health, without discovering a case of tuberculosis among these survivors. If the bacillus does act in the way supposed, its influence is certainly so slight that my observations have not discovered it.

There are many theories relative to its influence in causing disease which

our present knowledge of the bacillus does not qualify us to admit as worthy of our acceptance. I am very sure that much is believed now which time and further observation will prove to be without foundation.

DR. N. S. DAVIS, of Chicago: In forty-seven years' practice, during which I have examined and treated, both in private and hospital practice, a very large relative proportion of cases of phthisis, I have not seen any adequate proof of its contagion. Out of many thousand cases I have seen a *very few* in which the circumstances gave to them a strong appearance of communicability. It is much more in accordance with correct reasoning, however, to suppose that the circumstances were *coincidences* in those few cases, than to believe the disease contagious, when hundreds of well-marked cases are treated in hospital wards and families without the slightest apparent impression upon either the physicians, nurses, or others in daily contact with them. If it be said in reply, that the *contagion* (or *bacillus*, if you please) only takes effect on those who are *susceptible* or *predisposed*, which embraces only a small part of any given community, then I ask is it not both more logical and more in accordance with the facts of daily observation, to regard the final development as the natural outgrowth of the predisposition without regard to any supposed contagion? *Tuberculosis* in some of the lower animals has followed inoculation with so many substances that but little reliance can be placed on such inoculation tests as have come to my notice.

DR. BENJAMIN LEE said: The Lecturer has very properly said that with the demonstration or disproof of the contagiousness of tuberculosis, the *bacillus tuberculosis*, as the cause of the disease, must stand or fall.

He has also said that it is to the clinical observer that we must look for a solution of this question.

I wish to call attention to the grave difficulties which surround this problem from the clinical point of view, both in this country and in England. Tuberculosis may be said to be indigenous in both countries to a greater extent than elsewhere. The external conditions which favor its development are always at work. In any one family the conditions which would induce it in one member might very properly be credited with its causation in another member. In an acute infectious disease the relation of cause and effect is so apparent and direct as to force conviction. Not so, however, with a chronic infectious disease, the long period of whose development leaves room for the operation of natural causes apart from that of personal conveyance. One hesitates, therefore, very much to come to the conclusion that in a particular instance, the disease showing itself in successive members of the same family, as we constantly see it doing, has been communicated from one to another, rather than developed *de novo* in each by the same prejudicial influence. Again, the received doctrine of the intense heredity of this affection offers a most serious obstacle to the fair and dispassionate decision of the problem in any given case. When one parent, or even a brother or sister of a parent, has died of consumption, that fact is considered sufficient in the minds of most physicians to account for the appearance of tubercular or stiumous disease of any kind in any part or organ in any number of the offspring.

If, therefore, any other theory than that of contagion can be adduced to account for the manifestation of tubercle or phthisis in several members of the same household, all the preconceived ideas and early training of the family practitioner will lead him to adopt it.

It seems to me that there are only two conditions under which the problem can be successfully solved. First, that in which a healthy individual of good antecedents, as far as they can be got at, goes to live in the atmosphere of the room of a tuberculous patient both by day and by night. And secondly, that in which a tuberculous patient, or large numbers of them, go to reside in regions to which tubercle is not at all, or but very slightly, indigenous, and there associate on terms of great intimacy with healthy natives.

The first condition is offered to a certain extent when a healthy is united to a tuberculous individual in marriage, although even here the external conditions may be summoned to account for the affection developing itself in the former. Certainly there is a very respectable number of cases on record where a devoted wife, without the suspicion of a taint in her constitution, has shared the bed of her consumptive husband, and not long after his death, has been laid by his side in her last sleep, the victim of the same disease. Cases also are not wanting in which the sexes have been reversed in the order of the invasion of the disease with a like result, although the man, owing to the nature of his daily avocations, is rarely exposed to anything approaching the degree to which the woman is. The second experiment has been tried upon a very large scale in Europe. England, the home of phthisis, has for centuries been sending her consumptives by thousands to winter in Italy, whose genial climate exempts her population to a very great extent from this scourge as an endemic.

What has been the result? That the Italians, to a man, are thoroughly convinced of the contagiousness of phthisis pulmonaris. To such an extent is this true that, except in the large hotels of Nice and Mentone, which are really sanitariums for consumptives, as soon as an Italian host discovers that his guest is suffering from a suspicious cough, he adopts a thousand modes of annoying him until he is forced to seek quarters elsewhere. When a patient dies of the disease in an Italian house, his room is subjected to the most thorough disinfection. The apartment is fumigated, the walls are scraped and replastered, and no means spared which could destroy the supposed infection. Physicians, as well as laymen, share in this belief, unless their reading has led them to adopt the theory of foreign authors, that the disease is essentially inherent, which has been the case to but a very limited extent.

Now how could this belief have fixed itself so deeply in the consciousness of an entire people, unless it has been forced upon them by facts arising under their own personal observation; unless they had seen this disease, unknown in their family histories, introduced into their households by foreigners suffering under it.

This experiment on a grand scale seems to me worthy of the most respectful consideration. The lecturer appealed to the verdict of the clinicians, and

almost in the same breath, at the close of his valuable and interesting paper, told us that the clinicians in Germany were unanimous in their support of the germ theory of tuberculosis, while the doubters were to be found in the ranks of the pathological investigators. His conclusion, that the vote of bedside observers is overwhelmingly against the contagiousness of the disease, does not, therefore, seem to me to be warranted.

SYNOPSIS OF A LECTURE ON THE TREATMENT OF CHANCROID AND SYPHILIS.

Delivered January 9, 1884.

BY JOHN ASHHURST, JR., M. D.

IN introducing his subject, Dr. Ashhurst said that, as he believed that chancroid and syphilis had no connection with each other, except that they were commonly acquired under similar circumstances, it might seem strange that he should join them together in speaking of their treatment. The explanation, he said, was that two years ago, when he had had the honor to address the Society on the diagnosis of chancroid and syphilis, the Society requested that on some future occasion he would speak on the treatment of these two affections. In obedience to that request he had the honor to appear to-night.

THE TREATMENT OF CHANCROID AND SYPHILIS.

If any constitutional treatment is demanded in chancroid, it is such as is indicated by the general condition of the patient. Chancroid requires local treatment, but as syphilis is a constitutional affection, its treatment is constitutional or general. Local treatment is required for certain manifestations of syphilis, but the treatment, *par excellence*, is constitutional.

Speaking first of the treatment of chaneroid, we may recognize three plans which have been adopted.

First, that form of treatment which aims to abolish the whole thing at once, that is, by excision. There are certain maladies in which, by this plan, we can get rid of the disease entirely, as in the case of certain tumors. So a local disease, which has begun in one or more spots, should theoretically be removable by cutting away the diseased tissue. This plan has, however, been tried and found wanting. The great objection to it is that the wound almost inevitably becomes inoculated by the chancroidal matter.

and that the resulting sore is larger than the first one was, thus rendering the ultimate condition of the patient worse instead of better.

The second form of treatment, and that which I advocate, is one which aims not to remove the disease at once, but to favorably modify its future progress. This is the treatment by cauterization. By destroying the surface of the chancroidal ulcer, we remove its virulent qualities and leave a healthy granulating sore. The caustic application removes the tendency to spread, and converts the ulcer into a healthy granulating surface. In speaking of this tendency to spread, I refer to one of the most prominent features of chancroid, its auto-inoculability, in which it differs from the initial lesion of syphilis. Chancroid is auto-inoculable indefinitely, and I believe that cauterization very much diminishes, if it does not destroy, this property, although the pus from the chancroid is still contagious. It seems to lose, after cauterization, to a great extent, that quality which causes it to spread to other parts on the same person. In the choice of a caustic, my preference is for fuming nitric acid, applied by means of a piece of soft wood, such as the end of a match-stick. Another plan is to apply the acid by means of a glass brush, but I do not think this as desirable. Every cranny should be cauterized. Any part that escapes retains its quality of furnishing auto-inoculable pus, and the whole surface may return to its former condition; therefore, cauterization must be thorough if it is practiced at all. When the slough, produced by the caustic, separates, the surface soon granulates and heals, but the pus is contagious to the last. If the fear of pain deter the patient from submitting to cauterization, general anæsthesia may be properly employed, or the surgeon may first make an application of carbolic acid, which produces local anæsthesia, and apply the nitric acid afterwards. It may be necessary to repeat the operation.

There are other modes of effecting cauterization; one is the use of the carbo-sulphuric paste, recommended by French surgeons. This forms a crust, which I think is a disadvantage, as concealing the parts beneath. The solution of acid nitrate of mercury may be used, but if applied over an extensive surface it may cause salivation. It is not as well adapted to the purpose as nitric acid. The actual cautery also has strong advocates; it may be employed either with the simple hot iron, or with the Paquelin's

or the galvanic cautery. These modes of cauterization are effective in cases of serpiginous chancroid—in the latter I think the hot iron the best application that can be made. The material used by many practitioners a few years ago, the nitrate of silver, is inefficient, and, in my judgment, has nothing to recommend it. Then for the after-dressing, after cauterization has been employed, we can use plain water, or lime-water or black-wash, or a solution of salicylic acid, or what is known as the “nitric acid wash” (nitric acid $f\text{ } \overline{3}\text{ j}$; water O j), which is much used as a dressing in New York. When the chancroid is on a mucous surface, as in the female organs, or in any situation in which it is kept moist, a simple dry dressing of absorbent cotton or dry lint may be used; but where the chancroid is exposed, dry dressings are apt to become adherent, and wet applications are better. The dressing above all others which I think deserves attention, is iodoform. It is a comparatively recent remedy in these cases, and I think that it is the best application that can be made after thorough cauterization has been effected. It can be used in various ways, by simply dusting the finely-powdered drug over the surface, or as a wet dressing in the form of an alcoholic solution with glycerine, viz.: Iodoform $\overline{3}\text{ ss}$; alcohol $f\text{ } \overline{3}\text{ ii}$; glycerine $f\text{ } \overline{3}\text{ vi}$. Or it may be used in the form of an ointment, 15–30 grs. to the ounce, or as an ethereal solution which evaporates, leaving a thin film of iodoform over the surface. An old remedy, which formerly had great reputation in these cases, was aromatic wine, but I do not think it is as efficient as iodoform. Another remedy, which is quite a novel one, is resorcin, an article of the phenol series. Great advantage has been claimed for it. Pyrogallic acid has also been used, as has the subnitrate of bismuth and various other dry powders. In the female, dressings, of course, must be applied with the aid of the speculum.

In chancroids at the meatus, I commonly use a solution of nitrate of silver (30 grs. to $f\text{ } \overline{3}\text{ j}$), since the contraction after the use of nitric acid might be objectionable in this situation. At the frænum some special precautions may be required. Deep cauterization here may be followed by bleeding, and it has been proposed to prevent this by the previous application of ligatures, tying the frænum above and below the seat of disease, or by employing the actual cautery. For chancroids beneath the prepuce, when this can be retracted, the best plan is to cauterize the sores and dress

them in the ordinary way, either replacing the prepuce afterwards, or allowing it to stay retracted, as may be thought most convenient. If, however, the prepuce cannot be retracted, then the surgeon may inject a strong solution of nitrate of silver, or, which I prefer when it can be done, may pack the space between the prepuce and glans penis with lint saturated with a solution of nitrate of silver (gr. xx to $f\frac{3}{4}$ i). Whenever it is necessary to circumcise the patient, of course the wound should be cauterized, as it will otherwise become inoculated and itself converted into a large chancroid. As for urethral chancroids, which are very rare, cauterization cannot be employed, as increasing the risk of stricture; absorbent cotton may be used as a dressing, taking care to have a thread attached by which the dressing may be withdrawn. About the rectum and anus, chancroids may be treated by cauterization, with the subsequent use of emollient enemata and opium suppositories. For the phagedænic chancroid, constitutional treatment is desirable, as in all other cases of phagedæna. Opium—one grain at night and one grain in the morning—is, I think, more beneficial than any other single remedy. In some cases it may be of advantage to remove the surface of a phagedænic or serpiginous chancroid by scraping with a scoop, and then using as a caustic, bromine, permanganate of potassium, or caustic potassa; but I think that the hot iron is the best local remedy in these cases. Syphilization has been used for chancroid, but it is of no value.

In regard to the principal complication of chancroid, the bubo, it may be of two kinds, the simple or inflammatory bubo, which is nothing but an adenitis, or the true chancroidal or virulent bubo. I believe it to be impossible when a bubo first makes its appearance, for the surgeon to say of which variety it is. Of late years I have seen many more examples of the simple than of the virulent bubo. Whether or not this is because the disease, like syphilis, is gradually becoming a milder affection than it was formerly, I cannot say.

In regard to the treatment of bubo, the surgeon should enforce rest in bed, if possible. Then counter-irritation should be employed very thoroughly. The best way is that suggested by Mr. Furneaux Jordan, of Birmingham, by applying the counter-irritant to the "next vascular area." The theory is that by irritating an adjacent part, the blood is caused to flow away from that

originally affected. Counter-irritation is best effected by applying the tincture of iodine in the form of a broad horse-shoe around the inflamed gland, every day or every other day, so as to keep the part on the verge of vesication. The skin should, if possible, not be broken, but if it is so, some soothing ointment must be applied, and the use of iodine suspended for a few days. Over the bubo itself, the dressing which I have found most satisfactory consists of equal parts of belladonna and mercurial ointments; it is a simple resolvent and anodyne application, and is agreeable to the patient. I have also used an ointment of iodoform over the part, but do not think it as efficient as the belladonna and mercury; nor do I think the application of blisters as satisfactory as the use of iodine. Pressure is another remedy which may be properly employed when the bubo is not painful, but which is ill-adapted to the acute inflammatory stage. If it is to be employed, pressure may be effected by applying a shot-bag over the bubo while the patient is in bed; or by fastening a soft sponge over the part with a spica bandage applied with the thigh flexed on the trunk. If the bubo suppurates, of course it should be opened. Various plans have been suggested, but I do not think there is anything as efficient as a moderately free incision; and the direction in which this is made is a matter of considerable importance. I find that practitioners generally open buboes in the line of Poupart's ligament, but I think that an incision in the long axis of the patient's body is the best, supplemented, if necessary, by small transverse incisions on one or both sides. If the lips of the wound are kept apart, so as to allow the pus to flow out readily, the process of healing is much more rapid. Multiple punctures have been employed in opening buboes, and the introduction of a seton has also been suggested; in case phagedæna attacks the bubo, the use of the continuous hot bath has been proposed. My experience is here, too, in favor of the use of opium, locally and internally, and, if cauterization is necessary, the application of the hot iron. I think that there is an advantage, as regards the bubo, in a thorough cauterization of the original chancroid at the beginning. Bumstead and Taylor recommended that cauterization should be employed if it can be done in the first ten days; but if it is desirable in the first ten days, it seems to me to be proper at any period. These gentlemen believe that by early cauterization the patient will escape virulent bubo, and that even

if an inflammatory bubo exists, its course will be favorably modified. I am aware that a directly contrary opinion is held by some surgeons, who believe that the risk of bubo is increased by cauterization, but, as far as my own experience goes, it confirms the teaching of Bumstead and Taylor.

If the surgeon is satisfied that he is dealing with a chancroidal or virulent bubo, simple incision is not sufficient. Here suppuration occurs first in the periglandular areolar tissue, and it is of great advantage to enucleate the infiltrated glands before they become disintegrated and inoculate the surrounding tissues with chancroidal matter. If the case is not seen until the whole wound has become inoculated, then I would slit up all sinuses, remove the thinned, overhanging skin, and cauterize the whole surface with nitric acid, the patient being under the influence of ether.

The third plan of treatment, which is the fashionable treatment just now, is the use of simple dressings such as I have advised for the after-treatment, without employing caustics. There is no doubt that healing will, in most of the mild, superficial chancroids met with at the present day, ultimately take place without cauterization, but I think the cure will be more certain, more rapid, and more likely to be free from complication, if the chancroid be cauterized in the way that I have recommended.

Treatment of Syphilis.—Syphilis is a constitutional affection and demands constitutional treatment. The principal remedies are mercury and iodide of potassium. These have been given for many years, and yet it has never been satisfactorily determined in what way they produce their effects. Probably it is safest to say that they act by eliminating the syphilitic poison and producing absorption of the gummatous and inflammatory deposits. No doubt, according to modern theories, they might be supposed to act by destroying syphilitic germs, but that suggestion opens questions in transcendental pathology into which this is not the time to enter.

For the convenience of considering the treatment of syphilis, we may divide its course into the primary, secondary and tertiary stages.

The lesions of the primary stage are the initial lesion (or chancre) and the bubo which accompanies it. Now in regard to the treatment of primary syphilis, I believe that the surgeon will do well to put his patient under mercurial treatment, provided

that he is sure of his diagnosis. This view is opposed, however, by some authorities, for whom I have great respect. My practice is to give mercury; and the best form in which it can be given, in the primary stage, is the green iodide or protiodide. I have been in the habit of prescribing this preparation in pills with opium alone, or made up with a confection of opium as an excipient; it has the advantage that it can be used a long while without causing salivation, and it is, moreover, efficient. I think that this is the safest mode of treating syphilis in the primary stage, but no patient should be placed on a mercurial course unless the surgeon is well satisfied that syphilis is actually present.

In regard to the local treatment of primary syphilis, the principal point is cleanliness; but local treatment is not of much value. Iodoform may be used as a dressing for the chancre, as it may for the ulcerative lesions met with in the later stages of syphilis. Cauterization is of no service. I do not believe that secondary symptoms were ever prevented by cauterizing a chancre.

There is another form of treatment which has some evidence in its favor, and that is the excision of the chancre.

Until within a few years the view of surgeons was that a chancre should not be excised except under special circumstances, as when occurring on an elongated prepuce, but within recent years the excision treatment has been revived, particularly in Germany, and in this country it has been advocated by Dr. White and others. To those who, like myself, take the view that syphilis is a constitutional disease from the beginning, and that the initial lesion, chancre, is but its first manifestation, of course the excision treatment seems somewhat unphilosophical. I have no personal experience in this form of treatment, but the weight of evidence from what I have been able to read concerning it, seems to me to be against it. This, moreover, appears to be the prevailing view among the leading specialists in venereal affections in New York.

As regards the bubo of syphilis, no special treatment is required, though I have sometimes thought that I have derived advantage from the application of iodoform ointment.

In the treatment of the secondary stage of syphilis, of course mercury is the great remedy. Iodide of potassium is used by some surgeons in the primary stage, but for secondary syphilis all are agreed to use mercury. It should be introduced gradually, to

prevent salivation on the one hand and intestinal irritation on the other. I think the best way in which it can be used is by inunction. I recommend the patient to rub ordinary mercurial ointment, or an ointment of the oleate of mercury, into the inner side of the thighs, using fifteen grains each morning and night, half a drachm altogether in the course of the day. If this seem too much, the remedy can be suspended for awhile, and then used in diminished doses. Another good plan is to apply the ointment to the soles of the feet, wearing woolen stockings; the place of application should be frequently changed, so as to avoid the occurrence of mercurial eczema. Before each application, too, the skin should be thoroughly washed and dried. In cases of infantile syphilis, Brodie's plan of putting the mercurial ointment on the belly-band is a good one.

If a patient objects to inunction, then mercury must be given by the mouth. The old-fashioned blue-pill is one of the most efficient preparations, if it is given cautiously; or the iodide may be used, or the bichloride, which, however, I think less useful than the others. Mercurial fumigation is a good method of treatment in certain obstinate forms of cutaneous syphilis, but is too troublesome for ordinary employment. Another mode of administering mercury is by hypodermic injections, usually of from $\frac{1}{12}$ to $\frac{3}{8}$ gr. of the corrosive chloride, though almost any preparation of mercury may be used hypodermically. I do not think that this plan presents enough advantages to counteract its disadvantages, and believe that it should be reserved for exceptional cases.

For mucous patches, constitutional treatment must, of course, be continued, and as a local remedy, the solution of acid nitrate of mercury may be applied, being then followed by some simple dressing, such as black-wash, and iodoform afterwards. Another plan, recommended by Conradi, is to use a strong solution of nitrate of silver, and then to apply metallic zinc. For syphilitic sore throat, gargles of chlorate of potassium may be employed, or cauterizations with the Liq. Hydrarg. Pernitrat. ; or dilute hydrochloric acid may be applied with an atomizer. For syphilitic iritis, I have been favorably impressed with Carmichael's mode of treatment, which consists in the administration of oil of turpentine in large doses. I have often used this with great advantage, but have on the other hand sometimes found it to fail, and have had to come back to mercury. The oil of turpentine is

given in large doses ($\mathfrak{f}\mathfrak{3}\mathfrak{i}$) three times a day, in emulsion with gum and sugar, with a few drops of the tincture of opium to prevent strangury. The most important point in the treatment of iritis, however, is the local use of atropia. For alopecia, cantharidal washes may be recommended.

In the tertiary stage of syphilis, iodide of potassium is the chief remedy. Mercury is useful in the treatment of the dry eruptions and of interstitial orchitis, but not in the gummatous affections, where iodide of potassium is preferable. At the same time tonics must be given, as indeed in the secondary and primary stages also. An expectant plan of treatment has been suggested for syphilis, but it is not to be recommended, nor would I favor hygienic and tonic treatment by itself, though in connection with specific treatment it is of great value. A patient who lives a regular life, avoiding the use of tobacco and alcohol, and at the same time pursuing a proper course of treatment, has a better chance of recovery from syphilis than one who neglects hygienic measures.

In giving mercury for syphilis, there are two plans of proceeding: one in which small doses are given continuously for a long time, as particularly advised by Dr. Keyes, of New York; and the other, which seems to me more philosophical, in which the drug is given "*coup sur coup*," that is, in successive courses with intervening intervals. The doses should be moderate, and salivation should be avoided. The best way is to give mercury cautiously until the symptoms are relieved, or a few weeks longer, and then to suspend it altogether. Then, if there are any fresh symptoms, the administration may be renewed.

It has been proposed by Mr. Venning, as a test to determine when syphilis has been removed from the system, to examine the condition of the inguinal glands. If there is any induration remaining, the patient is still syphilitic.

Iodide of potassium may be used very freely in syphilis. Formerly, five grain doses were ordinarily given, but from eight to ten grains is now considered a fair dose to begin with, and in some cases much larger quantities must be employed. I am convinced, however, that the drug is often given in excessive amounts in ordinary cases of syphilis. I do not recommend large doses unless the disease fails to respond to smaller ones, or unless the symptoms, as in some cases of cerebral syphilis, are immedi-

ately threatening to life. The iodide may be given simply in water, or with the compound syrup of sarsaparilla, or with fluid extract of gentian, viz. :

Pot. Iod.,	gr. viii—x.
Ext. Gent. Fl.	℥ xv.

With enough water to make a teaspoonful. Iodoform has been given internally, and homœopathic practitioners have employed gold, but neither appears to have any special value. Sarsaparilla used to be looked upon as an important remedy for syphilis, but I have never found that it was of any use whatever. A remedy strongly recommended by the late Dr. Sims was stillingia. Dr. Taylor speaks favorably of the erythroxyton coca. Hot baths are undoubtedly of use sometimes in syphilis. For hereditary syphilis, mercury and iodide of potassium, in doses suited to the age of the patient, and combined with tonics, and especially iron, are of use. If a syphilitic woman is pregnant, she should undergo a mercurial course, in hope of preventing infection of the fœtus.

DISCUSSION ON THE TREATMENT OF CHANCROID AND SYPHILIS.

DR. J. WILLIAM WHITE opened the discussion by saying that there were a few points as to which he differed from Dr. Ashhurst, and to which he would take the liberty of alluding. The treatment of chancroid as proposed by Dr. Ashhurst, had some distinct disadvantages; in the first place it is very painful, and as a matter of fact it is not practicable to etherize one's office-patients, for the purpose of cauterizing chancroids. In the next place cauterization increases the liability to suppurating bubo. Cases of the latter complication had been much rarer in his practice since he had stopped the indiscriminate cauterization of chancroids, and, in fact, for several years past he had seen them chiefly in hospitals, where the cases have been cauterized before admission. Another strong objection to cauterization is that it is extremely apt to produce phymosis, and thus convert an exposed sore into a concealed one much more difficult to treat. If the prepuce were retracted and left so after cauterization of the sore, as recommended in some cases by the lecturer, paraphymosis, an even more annoying complication, would almost certainly result. Then, too, the diagnosis of the sore is rendered more difficult by the cauterization. He did not believe chancroid to be a specific sore in the sense that it is due to a special poison, producing only this form of ulceration, but thought it was due to pus contagion, owing all its distinctive peculiarities, if it could be said to have any, to the anatomical and histological peculiarities of the parts involved; he was convinced that his cases got well more rapidly when cauterization was omitted, than

when it was employed. As to cases which *should* be cauterized, they included in his practice only those sluggish sores which refused to take on reparative action under milder stimulus, and those phagedænic sores which are accompanied by rapid and dangerous breaking down of tissue. When a cauterant is necessary, fuming nitric acid was, in his judgment, the one to be preferred. The majority of cases do not require cauterization, and he thought could be best treated by observing the local indications; if the ulcer is inclined to be sluggish, a ten grain solution of sulphate or acetate of zinc, or a six grain solution of sulphate of copper, or a thirty to sixty grain solution of nitrate of silver, will often cause it to take on healthy action; if the ulcer is red with exuberant granulations, or surrounded by an inflamed area, then lead-water and laudanum would be indicated, or a weak solution of zinc in laudanum and rose-water. Iodoform will often effect a cure more certainly and rapidly than any other remedy, but the objection to it is the penetrating odor so offensive to many persons. He had tried all sorts of perfumes and pharmaceutical disguises without effect, and had finally adopted the plan of advising his patients to tie up a finger with some of the ointment, thus diverting suspicion from the true cause of its employment.

His experience in the Philadelphia Hospital had left him convinced that in the treatment of phagedænic and serpiginous ulcerations, bromine was the best local application; fuming bromine should be applied freely, not only to the surface of the ulcer, but also to all its interstices; it is afterwards covered by oiled lint. He had tried the so-called "horse-shoe" method of counter-irritation in bubo without much benefit, and favored pressure with a shot-bag or half-brick covered with flannel.

As to the suppurating bubo, he agreed with Dr. Ashhurst as to the mode in which the incision should be made in opening it, but said that curiously enough it was for a directly opposite reason. He made the incision parallel with the long axis of the body so that the lips of the wound would *not* gape. When the incision was made parallel with Poupart's ligament, whenever the thigh was extended on the body, the attachment of the fascia lata to the lower lip or wall of the suppurating cavity, and the attachments of the abdominal muscles and fascia to the upper wall, resulted in a wide separation of the wound, making a larger cavity to fill up with granulations and thereby delaying complete recovery very considerably.

In the treatment of sub-preputial chancroids he would hesitate, for the reasons stated, to retract the prepuce and cauterize, even if it were possible to do so, which would rarely be the case; he considered the paraphymosis, which would almost invariably result, a very objectionable complication; as to packing the space between the glans penis and the foreskin with lint, he believed it in the great majority of cases to be practically impossible, without exciting a degree of pain, inflammation and hemorrhage highly prejudicial to the patient; a certain amount of lint might be stuffed in, but he doubted that it could do anything but harm. In his cases of concealed chancroid—or of balanitic or herpetic ulceration with phymosis, he said he directed patients to clean the parts every two hours, injecting with a Taylor's sub-

preputial syringe, warm water and castile soap; then to wash away all the soap with plain warm water, and then finally to inject a solution of zinc in laudanum and rose-water, five to ten grains of zinc to the ounce of the mixture.

The local treatment of the infecting sore or true chancre was chiefly interesting in regard to the question of excision. If it were believed that the chancre was the first symptom of constitutional syphilis, it would, of course, be illogical to remove the sore with any idea of preventing systemic infection; but if, on the other hand, it were thought that the poison of syphilis, whatever it might be, found its way into the blood, and into the tissues from a point of original inoculation—the chancre—through the medium chiefly of the lymphatics—then it would be entirely philosophical to excise the sore in the hope of preventing constitutional disease. This was the view which he was inclined to favor.

The argument that in the *majority* of cases excision of the sore fails to prevent general disease, does not in the least affect this view of the matter. It is to be expected that in most instances a portion of the *materies morbi* would have passed beyond the reach of the knife before producing sufficient local symptoms to attract attention. Consequently the majority of cases fail and always will fail. The possible errors due to mistaken diagnosis, or to the *post hoc ergo propter hoc* method of reasoning, were, of course, familiar to all syphilographers, but it seemed manifestly unfair to include all the reported cases of successful excision of chancre under those heads. The only ways in which the true character of a hard chancre can be determined prior to the occurrence of constitutional infection are by its microscopical peculiarities and by "confrontation," or inspection of the person from whom it was obtained. Such cases must of necessity be very rare. He had been fortunate enough to have seen two of them—in both of which the sores, which were situated on the prepuce, had been excised and no constitutional syphilis had followed. They were obtained from women with marked secondary syphilis, and were examined microscopically by Dr. Simes, who found them to possess all the peculiarities of Hunterian chancres. Even two such cases, he thought, ought to be given great weight in a fair consideration of the question.

When, however, the sores are not seen at a very early stage of their development, or when they are situated on the glans penis so that their removal will cause deformity, or will give rise to much pain or hemorrhage, he thought the chances of preventing constitutional disease not sufficient to warrant the operation.

As regards the important question of the proper time for beginning constitutional treatment, Dr. Ashhurst's opinion did not seem to him to be consistent with the received facts of syphilography. It was an undisputed axiom that there is no absolute proof of the infecting nature of any given sore except infection itself, as manifested by certain constitutional symptoms. Mercury indefinitely delays or altogether prevents those symptoms, and its administration at the time recommended by Dr. Ashhurst would leave both patient and surgeon in doubt for all time as to the presence or

absence of syphilitic taint. As the proper treatment of syphilis involves a prolonged course of mercury, and the surgeon is compelled to insist upon abstinence from matrimony, or, if the patient is already married, the avoidance of conception, the responsibility is very great, and therefore nothing but entire certainty would justify the beginning of treatment. Where confrontation aids the diagnosis it might, perhaps, be allowable to begin sooner—but, although doubtless it is desirable to give mercury as early as possible, it has been abundantly shown that no great harm results from delaying its administration until the roseola or at least the general glandular involvement has established the fact of syphilitic infection.

After alluding to the various theories for using mercury, he said his own plan was to give it by the mouth, in the form of the protiodide. He gave four pills a day for two days; then six pills for two days; then eight pills for the same period, and so on until the patient's gums or his posterior molars became a little sensitive, or his saliva thickened and became more profuse; then the dose was usually divided by two, or if the toxic effect had been produced by a daily dose of only six pills, the subsequent or permanent dose (the so-called "tonic" or physiological dose) was reduced to two-thirds of that, or four pills. He thus determined in each case the quantity of mercury required to affect the new formations of syphilis, as it is well known that embryonic cells or tissues had less resistant power to either therapeutic or pathological forces than the normal cells. This dose was then continued, subject to increase in case of an attack of new symptoms, for a period of eighteen months to two years. Afterwards, he said, he put the patient on the mixed treatment of biniodide of mercury and iodide of potassium for six months. He then stopped and kept him under observation, returning to the mixed treatment if any new symptom appeared, and continuing it for several months.

Going over his case-books for the past seven years, rigorously excluding all hospital and dispensary cases, and all cases which had not been under his care and observation continuously from the primary sore down to the present time, he still had notes of one hundred and seventeen cases with whose personal and family history he was entirely familiar. These cases had all been treated upon this plan. Excluding the minor manifestations of syphilis, such as trifling mucous patches, occasional papular or squamous patches, etc., he found that among these persons there had occurred the following accidents which were fairly attributable to syphilis: four miscarriages, three of which were certainly due to syphilis, and one of which was doubtful; one case of perforation of the hard palate; one of epileptiform convulsions, and two of iritis; in one case the patient lost his penis from phagedænic ulceration supervening in a hard sore at the time of the secondary outbreak, but as he had gone on a long journey and neglected all treatment this should not properly be set down in the list; not one of all the others had an eruption which betrayed him to his family or friends, and the great majority did not lose a day from their various occupations of business, pleasure or professional work. In conclusion he said, that having had such results by the continuous plan of treatment, he

had not thought it necessary to make any extended trial of the intermittent plan, and consequently could not speak from any large experience with it; nor did he think that without further evidence upon the subject than he had yet heard, he would modify his present plan of treatment, though the intermittent plan had some distinguished advocates.

DR. S. W. GROSS thought that too much material had been presented for discussion, as there was enough to occupy four or five evenings; he would, therefore, limit his remarks to one or two points. In regard to the treatment of chancroid, he said he would have made about the same remarks as Dr. Ashhurst. He was in the habit of destroying the ulcer, with the triple object of preventing its increase, of preventing the auto-inoculability of the discharge, or the formation of other sores in the immediate neighborhood of the primary lesion, and preventing the formation of a bubo. His practice was to touch the sore with pure carbolic acid for its anæsthetic effects, and follow this with nitric acid. A dry dressing, in the form of absorbent cotton or picked lint, was then applied, and continued after the slough had come away if the resulting sore was not large; otherwise he employed a mildly astringent wet dressing, say three drops of nitric acid to the ounce of thin mucilage, or two grains of tannin and one-eighth of a grain of sulphate of copper to the ounce of water, or three grains of chloral to the ounce, if the surface be sensitive.

With regard to iodoform, on which the preceding speakers placed great reliance, he had only to say that he had no faith whatever in its action. The device of Dr. White of wearing an iodoform rag on the finger is so well known in this city, that it deceives no one. In his wards at the Philadelphia Hospital, during his connection with that institution, he had invariably found that iodoform dressings retarded the process of cicatrization in simple as well as specific ulcers, the granulations being rarely larger than pin-points. For this reason, with a view to induce a healthy granulating process, he had treated all such ulcers, left by his predecessor, with the nitric acid lotion, with the best results. His own empirical observations in this direction are sustained by some histological peculiarities pointed out by a German experimenter, whose name had escaped him, in a recent number of *Virchow's Archiv*. The writer shows that iodoform prevents the formation of polymucleated epithelioid cells and giant cells in granulations, and in this way accounts for the good effects of the remedy in scrofulous and tubercular granulations. From these experiments we may fairly infer that the formation of the elements essential to the rapid repairs of other granulating surfaces is prevented by the use of iodoform.

In reference to the treatment of syphilis, he agreed with Dr. Ashhurst that it should be by mercury, but he did not employ the remedy in the primary stage, because he wished to know what course each case was going to take. If the advent of the symptoms was masked or retarded, no one could tell from a prognostic standpoint what might occur afterwards. It not unfrequently happens that a man says he was treated with mercury for the initial lesion some months before, that no general symptoms have developed, and asks whether he can marry. In such a case as this the treatment

has delayed the appearance of the general symptoms, and the question can only be answered after several months of observation without any treatment whatever. For these reasons he withheld mercury in the primary stage. Before the appearance of general symptoms, he employed the blue pill where the protiodide did not agree. He was also in the habit, he said, of giving opium and tartar emetic to keep up the action of the skin, as the poison should, as far as possible, be eliminated in that way. He gave $\frac{1}{4}$ gr. of opium; $\frac{1}{30}$ gr. tart. emetic, and $\frac{1}{2}$ gr. of protiodide, or $1\frac{1}{4}$ gr. of blue mass, the dose being gradually increased until the tolerance of the patient was established. He thought that the mercurial treatment should be continued for months and months, with an occasional intermission, in accordance with the rules established by Keyes and other syphilographers.

DR. JOHN V. SHOEMAKER said that the use of mercury by the mouth, as recommended by the speaker, would not answer in all cases. In some the alimentary canal will not tolerate the drug; particularly is it the case in debilitated and broken-down persons, who seek treatment in the various dispensaries and hospitals. In others the mercury at times fails to make any impression, and in such instances it often passes out of the body by the secretions. He recalled a case of secondary syphilis, in which he administered for several months, first the protiodide, and afterwards the corrosive chloride of mercury, both in small and large doses, without obtaining the least impression, either from the drug or upon the lesions on the skin. In this instance he began later to treat the patient by the use of the corrosive chloride of mercury hypodermically, injecting one-tenth of a grain of the corrosive chloride of mercury, dissolved in water, deep into the subcutaneous-cellular tissue, and a cure followed within a few weeks. For the past three or four years he has followed, to a large extent, this plan of treatment with good results, and has never had any unpleasant symptoms follow mercury used in this way. If the needle is in a good condition, a gold one being preferable, and the operator inserts it deep into the cellular tissue, either in the superior or inferior scapular or sacral regions, abscesses will not follow. Dr. Shoemaker then illustrated, upon a patient having secondary syphilis, whom he had brought before the Society from the Philadelphia Hospital for Skin Diseases, the treatment of syphilis by the use of the corrosive chloride of mercury hypodermically. The eczematous condition of the skin, that sometimes follows the inunction method of treatment referred to by Dr. Ashhurst, can very often be avoided, by using, before the inunction of the mercury, a hot-air or steam bath. In treating patients in this manner at the Philadelphia Hospital for Skin Diseases, he always preceded the inunction with either one or the other form of a bath alluded to, and has caused the ointment to be better absorbed, and thus prevented the irritation to the skin. In the treatment of chancroid he has used the oleate of zinc, dusted over the surface, with most decidedly good impression upon the parts. It has the quality of being odorless, and has a slight stimulating, as well as an astringent, action.

DR. VAN HARLINGEN said that, with regard to the general treatment of syphilis, the ground had been so well covered by Drs. Ashhurst and White

that little or nothing remained to say. There are certain problems, however, which present themselves in connection with the management of late syphilis which had not been touched upon, and which are yet of great practical importance. One of these relates to the period during which treatment should be continued in cases seen for the first time in the later stages of the disease. For instance, a patient presents himself with a single late lesion or group of lesions, of the skin, or an ulcerated pharynx, a muscular involvement, or a nerve or cerebral lesion. A few weeks or longer may suffice to remove the outward evidence of syphilis, but how long should treatment be continued, not merely to prevent relapse in the original spot, but to prevent possible subsequent manifestation of the disease in some more important spot? The speaker said that his custom in such cases is to attack the lesion with iodide of potassium, beginning with five-grain doses, rapidly increased until the lesion yields or until the limit of tolerance is reached. When the lesion yields then mercury is to be added to the potash salt, and after a little time the latter is gradually withdrawn and mercury alone is administered for the space of at least six months after the disappearance of all signs of disease. This the speaker considered the safest method of treatment in late syphilis, and in practice he had usually found that it gives the patient permanent relief. Further observation, however, is required on this point, for if syphilis can be taken in hand at any stage and treated with the same probability of entire subsequent immunity as when treated from the beginning, the method and duration of such treatment should be settled. At present further exact investigations on this point are demanded.

Another important point is the realization of the fact that there comes a time in the history of late syphilitic lesions when specific treatment is of no avail. Specifics will remove the new-cell infiltration which constitutes the lesion, but a cicatrix may be left behind as in stricture of the oesophagus or rectum, or as a sequella of a gumma of the brain, which is as much a morbid product as the original syphilitic lesion, but is entirely unchanged by the administration of iodide of potassium or other specifics, no matter in how large dose these may be given. In such cases huge doses of remedies are vain. The dose should be rapidly increased, held for a short time and then diminished if found of no avail, or changed for some other form of treatment. Sometimes a simple tonic, as the tincture of iron, will cure when specifics have failed.

DR. W. R. D. BLACKWOOD remarked that he first saw syphilis and chancre on a large scale whilst stationed at Lexington, Ky., in 1863. His division was so affected by venereal diseases as to compel military supervision of houses of prostitution, and he made a personal examination of a large number of women twice weekly for over two months, all diseased females being removed for treatment to a special hospital. He was thus enabled to connect cases in the troops with the source of infection and to test with certainty the effect of treatment of syphilis prior to the advent of a chancre in men reporting a suspicious intercourse. In no instance did such treatment avert constitutional results. As three regiments of his division

were recruited from this city and Schuylkill County, he had additionally in many instances an opportunity of following up the after-history of men treated for chancre and chancroid. He invariably, during the nine years of his army experience, thoroughly cauterized all venereal ulcers of the genitals with fuming nitric acid, and with the exception of a year, during which he followed the prevailing plan of letting chancroids alone, he always does so, now being satisfied that the sore is not only thus more quickly treated, but that its tendency to auto-inoculate is controlled. Subsequent to cauterization, he used bismuth or similar mild dry powders. He frequently employed common *brown* sugar which acted nicely. He had tried as an experiment molasses and glucose, and also black and yellow washes, aromatic wine, and the usual routine dressings, but without good effect. Dr. Blackwood, after thorough trial, was satisfied that iodoform was of no value whatever, either externally or internally, in syphilis, chancroid or any other disorder. It was offensive to both patients and associates, and was used blindly by surgeons and physicians as a prevailing fashionable remedy. He let buboes alone unless they threatened suppuration, when he poulticed and incised freely. Potassio-tartrate of iron he valued highly, and the reason it failed with most surgeons was because they gave it in small doses—two to five grains. He never gave less than thirty grains—usually a drachm, four times daily. It is the most rapid blood-making ferruginous salt we have; does not constipate, and it certainly excels any other drug in controlling destructive ulceration. He added alum to it if diarrhoeal action set in.

Dr. Blackwood objected to mercurial inunction in syphilis as dirty, inconvenient, and causative of eczematous disorders. He did not use iodine or its compounds in secondary manifestations, but preferred bichloride of mercury throughout, even in tertiary. In the latter stage he used potassio iodide sometimes, especially in syphilis of the nervous system. He valued stillingia very much, *but it must be a good preparation*. He saw excellent results from it whilst stationed in northern Alabama and other southern States for five years after the war. The doctors outside the large cities there were as ignorant as the people generally, but the negroes and old women knew the value of stillingia, which is the basis of the "Cherokee cure." He always gives the compound fluid extract in full doses with the mercurial or iodide. Treatment was maintained for three years. He avoided salivation, and believed in the tonic effect of mercury according to Keyes.

Dr. Blackwood liked the hypodermic injection of bichloride, adding to it the chloride of ammonium if the patient did not strenuously object, although the operation is painful. He never had abscesses as a result, and if the needle is deeply buried this accident will not occur. He has seen many of his army patients since the war who are in perfect health, and who have now families free from constitutional taint. As the professional attendant of several houses of prostitution in this city for twelve years past, and from twenty-two years' observation, in which time he had treated a very large number of both sexes of whites, negroes and Indians, in military

and civil life, he was satisfied with his method of treatment as the simplest, the quickest, and the best to insure a permanent cure in any case.

DR. DE FOREST WILLARD said that the treatment of chancroids beneath the prepuce required extreme care. He had himself tried the plans of cleanliness and syringing and packing, and had carefully watched this form of treatment in the hands of others. "Buttonholing" and extensive sloughing had frequently followed even in hospital practice where strict surveillance was possible; in private practice, even when the individual realized the importance of strict compliance with directions, the environments were often unfavorable to frequent retracy. He decidedly preferred to have a sore, concerning whose size and tendencies he had seen many errors in diagnosis, open and visible. The removal of a portion of the prepuce, and, as was quite frequently possible, the cutting away with it of the sore itself, was a no worse operation at this time than at any other, as the cauterization of the raw edges, during anæsthesia, was painless, and much time was in the end saved.

An apparently contracted prepuce was, as the speaker had shown in a previous paper, an easily remediable condition in young children, but as he had then said, that whenever any symptoms arose referable to this stenosis, a "prepuce freely movable over a normal glans should be secured," so now when the youth has arrived at sufficient years of indiscretion to voluntarily contract a venereal sore, and had never been able to accomplish retraction, it was sufficient evidence that actual narrowing existed, and the sooner that a healthful standard was reached, the better would be the result. He certainly would not dare to retract the prepuce forcibly and cauterize, as Dr. A. had suggested. It is claimed that chancroids are not specific sores, yet all who speak from large experience in dealing with them, will acknowledge that they are vicious, destructive in their tendencies; that they are not self-limiting, but that they are checked only by the resistant power of the individual, either aided or unaided by remedial measures. Let the patient be non-resistant from any cause, and just in proportion to his disability will be the destruction. This is not the course of simple inflammation, hence it should be met actively and vigorously.

Each one has his favorite caustic; he preferred the acid nitrate of mercury, but would state that the worst case of salivation which he had ever produced, was caused by applying it to a chancre upon the os uteri, the liquid being permitted carelessly to trickle down the posterior vaginal wall.

In regard to tertiary syphilis, it had always been his practice to substitute a tonic course of treatment for the specific remedies, during one or two weeks of each month.

DR. PACKARD thought that the term "specific" might justly be applied to the chancre or soft sore, by reason of the peculiarities in its character and course, distinguishing it from the initial lesion of syphilis, as well as from other sores. He thought the excision of such soft sores unphilosophical, and injurious, as simply enlarging the extent of diseased surface; moreover, as the action was local only, and never productive of constitutional symptoms (of syphilis) there was no adequate object to be gained by

such a course. He preferred the milder caustics, and had used with advantage the stick of nitrate of silver, dipped in fuming nitric acid; this seemed to him more efficient than the nitrate alone, and perhaps less severe than the acid alone. He believed chromic acid, and the chloride of zinc, either in saturated watery solution or in paste, answered very well.

He advocated slitting up the prepuce to expose chancroids; believing that it is better to know just what we have to deal with, and to obtain ready access to the whole of the affected part. The preputial sore thus formed is under control from the outset, and need not give any anxiety.

As to sloughing sores, and their treatment by means of powdered white sugar, Dr. Packard stated that he believed he had been the first to use this article as a dressing in cases of hospital gangrene, at the suggestion of the late Dr. Le Conte, in 1864. The results obtained had been published in the *Am. Journal of the Medical Sciences*, for January, 1865. Dr. Packard was then acting as consulting surgeon to the U. S. A. Hospitals at Haddington and at Beverly, N. J., and a very large number of bad cases of this kind were under care at both places.

He thought the corrosive sublimate dressing recently proposed, might be of use in sloughing chancroids, and had had such favorable experience with eucalyptol in cases of gangrenous stumps and other wounds, that it seemed to him worthy of trial here also.

There were many other points of great interest in the subject under discussion, but he had nothing to add to what had already been said by other speakers.

DR. ASHHURST closed the discussion.

THE BACILLUS TUBERCULOSIS AND THE ETIOLOGY OF TUBERCULOSIS. IS CONSUMPTION CONTAGIOUS?

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(Continued from page 186.)

I wish to quote, however, some of the strongest affirmative evidence that exists in favor of the contagiousness of phthisis, in order to show upon what meagre clinical support this view is based.

The following case is related by Dr. C. Spriggs (one of the replies received by the English Collective Investigation Committee).

Miss R., aged 48, a dressmaker, living in rather a lonely cottage, had three apprentices, young girls of from 17 to 19 years, not related, from

three adjoining villages, who took turns to remain in the house and sleep with her, each one week at a time. During their apprenticeship Miss R. was taken with phthisis, of which she died. In less than *two* years afterwards all these apprentices died of phthisis, although it is said that in the family history of each no trace of phthisis existed; and the parents, brothers, and sisters of two of them are alive and well at the present time.

Another interesting case is related by Mr. G. F. Black (English Collective Investigation Committee), in which a perfectly healthy child, with a family history free from all trace of tubercle, is reported as becoming infected by a phthisical nurse and having died with profuse hæmoptysis, after the disease had run a rapid course.

Lindemann (Berl. Kl. Woch., July 25, 1888) related two cases of tuberculosis said to have followed the rite of infantile circumcision. The operator was himself subject to tuberculosis, and both children became ill and one of them died of tuberculosis.

Another instance is thus given (Dreschfeld, British Med. Journal, 1888): In a small town in Germany, where in the course of nine years only five children had died of acute tubercular meningitis, there happened in the course of nine months eleven deaths from that disease in infants all under six months. All these children were assisted into the world by a midwife who subsequently died of phthisis, and who had been in the habit, when attending a confinement, to breathe into the new-born child's lungs with the view of expanding them.

Lindemann (Verhand. Innere Medizin, Zweiter Congress, Wiesbaden, 1883) quotes the following:

A soldier at Strasburg was admitted into the hospital for rheumatism, and his bed was between that of two tuberculous patients. A few months after his discharge from the hospital he began to cough. He returned to his family and was pronounced phthisical by the physician. Gradually the mother, brother and father were affected by the disease. The father was attended by a neighbor, who was attacked and subsequently died, followed also by her husband.

Dr. Bela Cogshall, of Flint, Michigan, in a paper read before the American Public Health Association, 1882, quotes the following case after Dr. H. Weber:

A young man, with a well-established phthisical history, married four times, and lost all four wives of consumption. His first wife died after her third confinement; the second wife after a year of married life; the third wife after her second pregnancy; and the fourth wife after her first confinement. All four women are said to have come from healthy antecedents, and to have been "apparently" and "exceptionally" healthy prior to the time of marriage. Finally the much-married man died himself.

There is hardly any comment necessary. By side of the arguments and facts advanced in this paper such and similar evidence is entirely unsatisfactory, on account of the complete absence of direct scientific proof. On account of the isolated character of

the cases and the frequency of occurrence of phthisis, there is just as much reason in inferring a coincidence as a contagium. Furthermore, there is no proof that a family history of scrofula or phthisis or some other causes had been fully eliminated in the cases referred to.

On the other hand, daily observation and statistics show that there are thousands of instances which disprove the hypothesis of the contagiousness of phthisis. In multitudes of married couples where either the wives or the husbands died of phthisis, the surviving parties were known to have remained unaffected by the disease.*

V.—THE BACILLUS TUBERCULOSIS—ITS NATURAL HISTORY, MORPHOLOGY, DETECTION, HABITAT, SIGNIFICANCE, AND DIAGNOSTIC VALUE.

I will now speak about the bacillus proper, and will allude here briefly to its natural history, morphology, habitat, significance, detection, and diagnostic value.

The bacillus discovered by Koch, of Berlin, as is well known, is a vegetable organism, and belongs, according to Cohn's classification, to the group of filamentous bacteria (Desmo-bacteria), variety *Bacillus*.†

The tubercle bacilli form, according to Koch, a species of bacillus by themselves, and on Koch's authority as a *mycologist* we can accept this statement as correct until proven otherwise.

* Since the reading and discussion of this paper, Dr. William H. Webb, of this city, has kindly sent me his monograph, entitled "Is Phthisis Pulmonalis Contagious? Philadelphia, 1878." It presents an admirable and full résumé of that part of the literature in which the so-called communicability of phthisis is favored. Dr. Webb ably advocates that phthisis is contagious. The most interesting to me in Dr. Webb's paper is a letter of Professor Alfred Stillé, who, from his clinical observation, extending over nearly fifty years, relates the following:

"I have never seen more than *one case* in which it *appeared* to me that the disease was directly communicated. This was a mother, between fifty and sixty, whose husband many years before had died of consumption. She was herself in excellent, *tough* health up to the date of her daughter's last illness, which was with chronic phthisis with cavities. A day before her death the daughter's breath was very offensive, and the mother, who was lifting her to change the pillows, inhaled it. She spoke to me of the foul taste and acrid sensation in her throat, produced by the inhalation. Within a few weeks she began to cough, fell rapidly into consumption, and died after several months' illness. This is the only case of my own that appears to bear upon the affirmation of the question. On the other hand, *if pulmonary phthisis were often conveyed by contagion, the cases ought to be of daily occurrence, since the disease is the most frequent of all mortal diseases.*"

† The statements made by Beneke, Klebe and Schmidt that the bacilli are crystalline bodies have been withdrawn; while views to the effect that "bacilli" are to be identified with blood-fibrin, etc., were at no time taken into serious consideration by microscopists.

The tubercle bacilli present themselves as thin, slender rods, in length varying from one-third to the whole of the diameter of a human red blood-corpuscle; in breadth they do not exceed one-fifth to one-tenth of their length. They vary in size in different locations, and, according to observations made by myself conjointly with George Bodamer, my assistant, they vary also greatly in size in different artificial culture-media. In nearly dry soils they appear, as a rule, much smaller than in moist soils. They are blunt at the ends, and frequently contain unstained spores in varying number which give them a beaded appearance that might be (and has been) mistaken for short torula chains of micrococci. The rods are sometimes slightly curved, and they frequently appear in pairs, forming a V-shaped figure; occasionally the rods are seen crossing one another. Often they appear within animal cells in tissues and other matters which they invade, quite isolated and scanty, so that there may be seen only a few bacilli in a whole microscopic field. Sometimes they occur in large, dense masses, particularly so within and around cheesy masses in lymph-glands, and in the cheesy fragments met with in the contents of lung-cavities, as Koch himself first pointed out.

It may be of interest to note that tubercle bacilli may considerably multiply in sputum when it stands in a bottle for some time, as first observed by Bodamer in my laboratory. Williams, of the Brompton Hospital for Consumptives, records also that he has seen the bacilli multiply in sputum after standing in a warm room for ten days.

For demonstrative purposes it is well to inspissate tuberculous sputum or to dry it (as I have seen in Koch's laboratory) for examination; it is then moistened with water, and it will then show more bacilli than when fresh.

The methods of detecting the bacillus are so well known that I will not consider in this communication the merits of the different dyes employed. Moreover, success does not depend upon the method or the dye, but mainly upon the skill and the accuracy of the dyer.*

As generally known, the principle in staining bacilli rests upon the fact that bacteria absorb and retain aniline dyes more readily

* To detect bacilli is a very simple matter, although by far not as easy as to prepare a specimen of urine and to find the all-important tube-casts; and yet how many physicians (even those perfectly familiar with microscopic technology) will be sure when they discover tube-casts, if they attempt to examine the urine at all?

than the surrounding animal organic materials do. When sputum dried upon a glass cover, or a section containing them, is well stained, for instance, by aniline violet and then washed in very dilute nitric acid, only the bacilli will retain the dye, while all the rest of the organic material composing the specimen will be decolorized and may readily be stained by some other dye without modifying the violet color of the bacilli.*

A magnifying power of four hundred diameters is nearly always sufficient to detect stained tubercle bacilli. In fact, if we found that where we failed to find bacilli with a good one-fifth objective, neither our one-twelfth Zeiss oil immersion lens nor the Abbé's condenser would reveal any when used (as we always do) for control. If the bacilli are very numerous (as sometimes in lymphatic glands) a mass of them may be recognized easily by the naked eye in a well-stained section as a small stained speck.

Occasionally bacilli may also be seen when unstained. Baumgarten† discovered the same tubercle bacillus simultaneously with, and independently of, Koch, in unstained caustic potash preparations of tubercle tissues. Koch‡ also states that tubercle bacilli may be readily seen, especially in artificial tubercles when simply teased in water, or preferably in blood-serum. We have also observed tubercle bacilli, without resorting to staining, in cultures such as chicken *bouillon*, identifying them subsequently by means of the usual staining process. In stained preparations too much washed in acid, or in specimens ill preserved, a part or

* The staining fluids for bacilli we more commonly use are those after Ehrlich's formula, slightly modified:

First Stain.—Watery saturated solution of aniline oil, *five parts*; alcoholic saturated solution of aniline violet, *one part*; mix and filter.

Second Stain.—Watery solution of either vesuvin or of Bismarck brown; filtered.

Direction for Preparation of Sputum and Order of Staining.—Sputum in thin layer smeared upon glass cover and well dried; immerse: (a) into first stain for twenty-four hours (rapid staining being not reliable in doubtful cases); (b) into dilute nitric acid (one to five parts of water) for two or three seconds; (c) wash in alcohol; (d) into second stain for two minutes; (e) wash in water and then in alcohol; (f) dry it and mount in Canada balsam or glycerine. Failures to detect bacilli will occur; first, when specimen consists of salivary mucus instead of expectorated material; second, when sputum too thick or too thin smeared upon cover; third, when not enough heated in drying, or when burned; fourth, when too long in acid; fifth, when too much washed; sixth, when bacilli are absent; seventh, when not recognizing them.

For preparations to be kept, and for tissues, the fuchsine dye is preferable, and certain modifications of method necessary.

† Med. Centralblatt, 1882, No. 15.

‡ Berliner klin. Wochenscher., 1882, No. 15.

all of the tubercle bacilli may also be seen decolorized, though still quite distinctly visible.

Tubercle bacilli are, as a rule, motionless as seen in stained preparations made from the substances they inhabit; but the observations of Bodamer and myself appear to show that the bacilli of Koch may also have an actual (not communicated) motion when for some time cultivated in liquid media. But at the same time it was observed that the development of the cultures was not as extensive in liquid media (*bouillon*) as in a solid medium (coagulated blood-serum). Conversing with Koch on this point last summer, he remarked that this was quite possible, and suggested that perhaps the bacilli in their movable state acquire flagelli or cilia at the ends, although he had not yet made such observation. Koch, quite properly, does not seem to consider that motion is an invariably differentiating feature for bacteria.

In cultures (coagulated blood-serum being the preferable nidus) the tubercle bacilli grow as a dry, scaly, tortuous, whitish gray mass, spreading themselves exclusively on the surface. The growth is very slow, and is favored by a temperature of 30° to 40° C. (86° to 104° F.).

Dr. Koch kindly demonstrated to me a number of specimens of bacilli, and in particular the appearance of these bacteria exhibiting under low amplification the peculiar S-like figure in the growths in masses. Koch seems now to lay more stress upon this low-power appearance and upon the pathogenetic properties of the bacillus tuberculosis as a distinguishing feature from other bacilli than upon the color test. During the conversation he admitted that some other bacilli may also yield the same microchemical reaction as the tubercle bacilli, but insisted that the latter bacilli cannot be stained brown. The failure of the tubercle bacilli to take the brown stain, he said, was the reason that they cannot be well photographed (blue and red-stained objects not being suitable for photographing). He obligingly explained to me the details of his methods and the determination of the value of cultures. I learned from him that those cultures in which the bacilli had no spores are not capable of propagation, nor are they fit for inoculation of animals.

Klebs, to whom Koch had given some of his cultures of tubercle bacilli, having declared that they also contained micrococci,

Koch presumes that Klebs has misinterpreted the granules of the coagulated blood-serum (in which they grew) as micrococci. I can testify that bacilli alone were present in those cultures of Koch which I had the opportunity of examining. This is also true of a bacillus culture in a flat salt-dish obtained from Koch's laboratory by Dr. Shakespeare, of this city; this culture was still perfectly pure (and free of micrococci) when examined by Dr. Shakespeare and myself, three months after the arrival in America.

Concerning my own bacillus cultures which I recommended last autumn (and which are now more often successful than before I went to Berlin, through the use of the complete outfit of Koch's apparatus, supplied by the University of Pennsylvania), I will report later. But it may be said that, even under the most favorable conditions, to obtain success with the tubercle bacillus culture is at times a difficult task.

Before leaving this part of the subject, I must express that I owe many thanks to the director of the German Imperial Board of Health, Dr. Struck, and to Dr. Koch and his assistants, for the very liberal and kind treatment which they extended to me in their laboratory; also for allowing me to study the whole working of their famous institute, and demonstrating to me their methods of work, the construction of their ingenious apparatus, and permitting me to exercise all the important manipulations in bacteridian studies after Koch's method; and, furthermore, for allowing me to prove that I had succeeded also in staining and recognizing the tubercle bacillus before I went to them.

I cannot blame Koch for not demonstrating to me how to produce genuine induced tuberculosis with his bacillus within eight days, a favor which he extended only to Watson Cheyne;* not because I have not yet the "faith" in the infallible action of the tubercle parasite, but because Koch was then working at the subject himself, and does not consider the task as much finished as his overzealous followers do. I was moreover informed, while in his laboratory before leaving Berlin, that no one besides himself and his assistants ever worked in the laboratory on the tubercle bacillus beyond staining tissues, sputa, etc., containing it. Besides, the cultivation of the tubercle bacillus takes a longer

* See "Practitioner," April, 1888, page 249.

time than usually is allowed to outsiders who come to be instructed in Koch's laboratory.*

The habitat of the tubercle bacillus.—After reading most of the numerous compilations in reference to the present standing of the tuberculosis question, it would seem that Koch has established that his tubercle bacillus is always associated with tuberculosis, and with the diseased products and the various excreta in this disease—and in this disease alone. Since Koch's publication appeared, a number of observers, authoritatively and otherwise, assert the invariable presence of the bacillus in *all* tubercular products; and, further, it is claimed as a proven matter that the bacillus is found in the beginning of the disease, viz., in the youngest tubercle tissues.

This is, however, not in accordance with the facts. Neither in Koch's own publication nor in the records of any microscopist (when the original papers are examined) is the invariable presence of the bacillus in tuberculous lesions or excretions, and its absence in non-tuberculous matters, either clearly shown or proven. Moreover, the authors of nearly all the literary productions are in favor of the contagiousness of tuberculosis, and they disregard, as a rule, the negative evidence.

The question of the occurrence, and partly that of the significance, of the bacillus called by Koch the *tubercle bacillus* in tuberculous lesions divides itself into several parts and hinges upon the results of the following investigations:—

1. The examination of tissues affected by tubercular disease for the bacillus; and, if present, the time of its occurrence.
2. The examination, *intra vitam*, of blood of tubercular patients.
3. The examination of the products discharged or eliminated with the excretions by individuals suffering from tubercular disease.

* I found that the "pilgrims from all nations" who (through influence brought to bear upon the authorities) succeed in being admitted for awhile to Koch's laboratory, are instructed principally in the most rudimentary manipulations of mycology; and to most of them the assistants have to first point out what a bacterium looks like. Besides these "pilgrims," the German Government sends regularly young sanitary officers to be instructed in mycology. Of course, this is a very useful matter to the "pilgrims" and to the young sanitary officers, even if only one out of twenty-five ever devotes himself to mycology; but it is no beneficial matter to Koch and his kind assistants, who, through this constant interruption, are terribly interfered with in their scientific work. In fact, the working of the Imperial Laboratory is sometimes completely delayed in this way, as it was last summer, during the Hygienic Exhibition. Yet the beneficial influence upon sanitary science which this excellent institution exerts is very great.

4. The examination of air, viz., of the breath of phthysical patients, and of the air of sick-rooms and hospitals generally.
5. Comparative studies in animal tuberculosis.
6. The occurrence of bacilli in lesions and substances other than tubercular.

I will state now, briefly, what so far have been the results of the investigations upon these points.

1. Tubercle bacilli have been detected quite often in the various forms of tubercles of lung, and in scrofulous and tuberculous lymphatic glands; and likewise, although not so frequently, in tubercles of the various serous cavities; and in tubercular ulcerations of the mucous membranes and the skin. But it must be noted that only a few microscopists have recorded examinations of tubercle tissues for bacilli, and among these there was *not one* who did not meet with a case or a certain number of cases in which tubercle bacilli were either totally absent in the tissues or only present in some of the tubercles. The great bulk of bacillus work done comprises merely examinations of sputum.

The facts concerning bacilli in tissues are as follows:

Koch* found bacilli in the majority of tuberculous lesions he examined, but still not in all, as he states himself; he only *supposes* that his bacilli, even if they escape observation, are still present in all cases and in all tubercles. His proposition, however, that in some tuberculous lesions only unstained spores of tubercle bacilli are sometimes present, or that bacilli may be invisible, and not taking the staining when dead, or even may be absent if the tuberculous process comes to a "stand-still," is of course purely hypothetical. There is still another good reason for the assumption that the proportion of non-bacillary tubercles may be much larger in Koch's own examinations. As Koch says himself, he pre-eminently recognizes only such structures as tubercular which contain his bacillus, regardless of their morphology otherwise; it is therefore possible that he may have innocently excluded a number of non-bacillary tubercles from the list of his tubercle records. Koch himself, however, says that he failed to detect bacilli in some scrofulous glands and in two cases of tubercular synovitis, and further admits the prevalence of bacilli in degenerated tissues.

As far as examination of tubercle *tissues* for bacilli is concerned,

* Berl. Klin. Wochen., No. 15, 1882.

only the following observations besides those of Koch are recorded (as far as is known to the writer), and with the following results:

Dr. Geo. M. Sternberg, U. S. A.,* who is a man recognized as a competent mycologist, here as well as in Europe, failed to find tubercle bacilli in the lesions of several cases of tuberculosis.

Heneage Gibbes† also failed to discover bacilli in a number of tubercles, particularly in the reticular form; in fact, he had met several times with non-bacillary tuberculosis. Gibbes states‡ that "he had examined the lungs of guinea-pigs which had become tuberculous after being kept in the air-shafts of the Brompton Hospital for Consumptives, and had found no bacilli in them; and he knew of an instance in which a guinea-pig, inoculated with sputum from a case of phthisis, presented a glandular abscess in the thigh which abounded in bacilli, whereas the internal organs, although full of tubercles, did not yield a single bacillus."

I do not think it likely that Heneage Gibbes, by his large experience and universally recognized skill in bacteria stainings, would fail to discover bacilli if they had been present.

Watson Cheyne,§ whose anatomical conception of tubercle is inseparable from the bacillus, of course says that non-bacillary tubercles, like the above, are no tubercles at all. Hence his statement, that in all tuberculous structures (that is, in all structures which *he* calls tubercle) the bacilli are invariably present, is, from his standpoint, perfectly warrantable. He also confirms the fact that recently-formed tubercle nodules made up of young lymphoid cells, are, as a rule, without the bacilli, while the older tubercles, always containing epithelioid cells (on account of retrograde changes), usually do contain bacilli. Now, Watson Cheyne, in this connection, with great self-confidence propounds, "The bacilli being the cause of this disease (tuberculosis), only the nodules containing epithelioid cells are tubercle."¶ Still Watson Cheyne has expressed surprise¶ that "very extensive tuberculous processes may be found in animals with only very few bacilli."

T. M. Prudden, of New York,** who made extensive and excellent morphological studies in reference to the occurrence of the bacillus in tuberculous lesions, *failed to find bacilli in any part*

* Phila. Med. News, 1882.

† London Lancet, February 24, 1883.

‡ Lancet, February 12, 1883.

§ Practitioner, April, 1883.

¶ See p. 309, loc. cit.

¶ P. 316.

** Med. Record, April 14, and *ibid.*, June 16, 1883.

of the body in three cases of profuse tuberculosis. In one case of Prudden's the tubercle bacilli were abundant in the walls and edges of a lung-cavity and its immediate vicinity, while no bacilli could be found in the diffuse and miliary tubercles of the rest of the body. Prudden further states : " In a large proportion of the cases in which bacilli were present they seemed to have a decided predilection for tubercle tissue in a degenerated and disintegrating condition, either cavities in the lungs, cheesy and breaking-down areas, or tubercular ulcers ; although present with great frequency in small numbers in well-formed, intact, tubercle tissue." . . . " The bacilli were present in greater abundance in the respiratory organs and intestinal tract than in other parts of the body less directly in communication with the external world. It is further evident that in nearly every case there are many miliary tubercles of all forms, and in many cases much diffuse tubercle tissue from which the bacilli appear to be entirely absent."

Spina* did not succeed in detecting the bacillus in a number of cases. Even if the number of Spina's failures to see the bacillus should be larger than in cases of other observers, Koch's favorite demolishing argument that Spina and all others who failed to detect the bacillus in any case do not know *what* that parasite of his looks like, is entirely unjustifiable. Moreover, Spina's work was controlled by no less an authority than Stricker, of Vienna, and the correctness of the results of the investigation in its essential parts is vouched for by Stricker.

Cornil and Babès† detected the bacillus in the lesions of a number of cases of tuberculosis ; but they also showed that bacilli are totally absent in some cases, and not constant in otherwise typical tuberculous lesions.

Malassez and Vingal,‡ from the results of observations of their own, state that there seems to be no doubt that true tuberculous lesions occur which possess very few or even no tubercle bacilli.

Fräntzel,§ in a discussion before the Berlin Medical Society, stated that he found a number of scrofulous (tuberculous) ulcers and lymph-glands not to be " bacillary."

* Studien ueber Tuberculose, Wien, 1883.

† Le Progrès, Méd., 1883.

‡ Le Progrès, Méd., No. 20, 1883, and in a second communication quoted from the Lancet, December 15, 1883.

§ Berliner Klin. Wochenschr., December, 1883.

C. Macnamara* reports a case of primary tuberculosis of bone and of the marrow of bone, in which no trace of tubercle bacilli could be discovered in any of the lesions.

George Bodamer, having succeeded in staining and demonstrating the bacillus in sputum and in tissues in the spring of 1882 (immediately after the announcement of its discovery and the method of its staining by Koch, and probably prior to any one else in America), and having worked with me nearly incessantly in bacillus stainings and cultures ever since (including also a certain time in the pathological institutes in Germany), also failed to detect the bacillus in a certain number of typical tuberculous lesions.

As will be seen from my report, I found tubercle bacilli to be absent, or I could not detect them, if this expression is preferred, in four cases of primary peritoneal tuberculosis, in two cases of primary tubercular pericarditis, in one case of tubercular joint-disease, and in several cases of miliary tuberculosis; this does not include some cases of induced animal tuberculosis which did not show bacilli.†

Dr. Lawrason, of New Orleans, who worked with me last spring in the pathological laboratory of the University, and who had demonstrated his skill in staining bacilli in tissues before the Pathological Society of Philadelphia and elsewhere, also found tubercle bacilli wanting in some of the most typical tuberculous lesions.

Weigert, Bollinger, Baumgarten, Ziel, Councilman, Schuchart, and Krause, and Koch's own assistants are yet to be mentioned as having recorded a few examinations of tuberculous tissues for bacilli with positive and varying results, but detailed statements of their investigations in this direction are not known to me.

The direct conclusion to be drawn from the total evidence relating to bacilli in *tissues* just quoted, is that tubercle bacilli are not invariably present in even typical tuberculous lesions; furthermore, that none of the investigators brought forward any proof or evidence that the bacilli are present or appear in the beginning of the disease. On the contrary, the results of the

* Brit. Med. Journal, December 15, 1883.

† At this point I wish to correct an impression which a certain statement in one of my former communications on this subject seemed to convey, namely, that bacilli are invariably present in tuberculous products.

investigations of all observers, including those of the discoverer of the bacillus himself, point plainly towards establishing the fact that tubercle bacilli inhabit pre-eminently disintegrated tissues.

2. *Examinations of the blood and lymph intra vitam* of patients suffering from tubercular disease, which in my opinion would be quite an important matter in the study of tuberculosis, are not recorded by any observer. All attempts which we made in examining the blood of tuberculous patients during life, gave, as will be recorded later, negative results. It is true that we observed in specimens post-mortem some blood-vessels filled with thrombi containing a few bacilli. Further, there are records by Cornil, Weigert, Ponfick and Koch relating to bacilli observed post-mortem in the walls of veins, of large lymph-ducts, and of arteries in tuberculous cases. As to the route and manner by which the bacilli gained entrance to these places, inferences might be drawn, but no definite conclusions can be arrived at until the bacilli have been observed during life in the blood or lymph. I will not touch upon this part of the question at present.

The blood from cases of hæmoptysis as expectorated, has been examined by Hiller* and Williams,† and bacilli discovered, but no inference from this can be made as to the bacilli in the circulating blood.

3. *Examination of products discharged or eliminated with the excretions* by individuals suffering from tuberculosis has been practiced quite extensively and by a number of observers, and especially examination of phthisical sputum. To these sputum examinations I will return immediately.

There are a few investigations recorded in reference to tubercle bacilli in the fæces, in discharges from the ears and in those from the nose, and in urine voided by patients affected with local tuberculosis of the pertaining parts. Tubercle bacilli were often detected, and thus a diagnosis of tubercular enteritis, tubercular otitis, and tubercular meningitis from bacilli in nasal discharge, and tuberculosis of the urinary tract, was made.

The tuberculous nature of ulcers, of synovitis, and of surgical lesions of various locations, is claimed to have been occasionally

* Deutsche Med. Wochensh., No. 47, 1882.

† London Lancet, February 24, 1883.

settled (?) in this way.* But, on the other hand, the discharges from some typical tuberculous lesions failed to show bacilli.

Damsch† claims that tuberculosis of the genito-urinary tract can be diagnosed by inoculating a drop of urine from such a case into the anterior chamber of the eye of a rabbit, the operation being promptly followed by iris-tuberculosis in the animal. This latter observation, however, I believe, requires confirmation.

The examinations of sputum, practiced now probably by all microscopists in the world, has proved to be of much more value. I will quote the observers who made and recorded more or less numerous examinations of sputum, and the results and conclusions they arrived at, to show that there are some points which are misinterpreted by some clinicians and others.

Koch‡ does not claim that sputum from every phthisical case contains bacilli; he met with cases without bacilli in sputum. He did not find, however, bacilli in cases said not to be tubercular.

Ehrlich§ records twenty-six cases of phthisis in which bacilli were invariably present in the sputum; in other lung-affections similar bacilli were not found.

Balmer and Fräntzel|| examined one hundred and twenty cases of phthisis for bacilli with positive results, and came to the conclusion that the quantity of bacilli was in direct proportion to the gravity of the disease, and that the bacilli were larger and often contained spores in acute cases, and were smaller in size and quantity in chronic cases. They never saw bacilli in the sputum of cases other than phthisis. They also quite properly conclude "that the sputum affords to bacilli a more favorable place of growth than does the still living lung tissue," because they found bacilli to be extremely scanty in the tuberculized lung tissue surrounding a cavity, while the contents of the latter and cheesy degenerated parts of the lung were crowded by them.

Heron¶ records sixty-two cases of examination of phthisical sputum, in which bacilli were constantly present.

D'Espine** records examination of sputum from twenty-five

* Schuehart and Krause in Volkmann's Clinic, Chirurg. Centralblatt, 1883.

† Deutsch. Arch. f. Klin. Med., 1882.

‡ Loc. cit.

§ Deutsche Med. Wochenschr., No. 19, 1882.

|| Berliner Klin. Wochenschr., No. 45, 1882.

¶ London Lancet, February 2, 1883.

** London Lancet, January 13, 1883.

cases, but could not confirm the correctness of the assumption that the bacilli stand in any relation of quantity to the gravity of the disease, although he affirms that they are constantly present.

Williams,* having examined the sputum of one hundred and thirty cases for bacilli, with only three negative results, concludes, however, that there was "no definite ratio between the activity of the disease and the number of bacilli, although there were few in cases where the disease was quiescent."

Ziehl † found bacilli invariably present in seventy-three cases of phthisis examined; Dreschfeld, ‡ in forty-six cases; and Gradle and Woltmann, § of Chicago, in thirty-five consecutive cases.

Kowalsky || claims to have examined the sputum of six hundred cases of phthisis, with bacilli nearly invariably present.

Chiari, ¶ in a number of cases examined, never failed to find bacilli.

Detweiler and Meissen** examined eighty-seven cases of phthisis, finding bacilli in all but two. Although bacilli were more numerous wherever there was great destruction of lung-tissue, they did not observe any definite ratio of bacilli in sputum to the gravity of the disease. The presence of elastic tissue in sputum they consider as significant for diagnosis and as constant as that of bacilli.

S. West †† found bacilli present in every case of phthisis which he examined, though in some cases they were in such small numbers as only to be found after repeated and very careful examination. He further adds: "The more cheesy matter or fluid from a cavity there was in the expectoration, the more bacilli we might expect to find; consequently, in a case of acute tuberculosis, before breaking down of the lung, we should expect to find none." He also states that there appeared to be but little variation in the size of the individual bacilli in different cases, although bacilli in acute cases appeared to contain spores.

R. S. Smith †† records seventy-seven cases in which he had made examination of sputum; of these, forty-nine were from "tubercular phthisis," and invariably showed bacilli; the remaining twenty-eight, comprising various other affections of lungs, some

* London Lancet, February 24, 1883.

† Deutsche Med. Wochenschr., N. 5, 1883.

‡ Brit. Med. Jour., February 17, 1883.

§ Med. News, 1883.

|| Wien. Med. Presse, February 24, 1883.

¶ Wien. Med. Presse, No. 1, 1883.

** Berlin Klin. Wochenschr., N. 7 and 8, 1883.

†† London Lancet, February 10, 1883.

‡‡ British Med.-Chirurg. Jour., July, 1883.

of them closely simulating phthisis, did not show bacilli. The affections examined with negative results were such as "chronic bronchitis, bronchiectasis, chronic syphilitic pneumonia, slight hæmoptysis with no evidence of any disease, chronic pleuropneumonia with dulness on percussion and copious purulent expectoration, chronic pleurisy, apex pneumonia with subsequent breaking down from gangrene and with cavity (?), sarcoma of lung, gray hepatization, congestion from mitral disease, diabetes with bronchitis, two cases with strong family history of phthisis, cough with purulent expectoration, but with no evidence of local disease in lungs," etc. Bacilli were also wanting in "slight phthisical cases when the patients were rapidly recovering." I think, however, that errors in physical diagnosis can by no means be fully excluded here.

Heneage Gibbs,* from his extensive observations, states that the sputum did not show bacilli in some cases which upon the autopsy-table showed the lungs riddled with tubercular masses; he explains that the patient died before the destructive process had gone far enough to cause the bacilli to be ejected.

Whipham† records twenty cases which he studied in relation to bacilli in sputum, and made the observation that the bacilli disappear from sputum at times when the condition of the patient improved.

The report upon the examinations of sputum for bacilli from the pathological laboratory of the University of Pennsylvania will embrace the results from nearly two hundred cases of pulmonary diseases observed. These show that bacilli in sputum are diagnostic, but not prognostic, in phthisis; that the old-fashioned test, the presence of pulmonary elastic tissue in sputum, is a very reliable one, gangrene and abscess being so easily excluded; and, further, that the absence of tubercle bacilli in sputum proves nothing.

Prudden‡ found bacilli in sputum in forty-six out of fifty-eight phthisical cases examined.

Guttmann§ and Pfeiffer|| met with many cases without bacilli in sputum.

Spina and Stricker¶ met tubercle bacilli in simple bronchiectasis, bronchitis, croupous pneumonia, etc.

* London Lancet, February 24, 1883.

† London Lancet, February 10, 1883.

‡ Med. Record, April 14, 1883.

§ Berl. Klin. Woch., N. 52, 1882.

|| Ibid., N. 3, 1883.

¶ Loc. cit.

Sattler, in the translation of Spina's book,* page 164, adds the record of an autopsy of a case of similar nature mistaken for phthisis on account of bacilli in sputum.

Kundrat† related a case which occurred in the spring in Nothnagel's clinic, where a diagnosis of tuberculosis was based upon the detection of bacilli; but, post-mortem, the case proved to be one of chronic catarrh with bronchiectasis. He also mentioned a case, under Prof. Schrötter, where bacilli were repeatedly found by himself and others, and the necropsy showed only bronchitis and emphysema. Hence he was not disposed to admit that the discovery of bacilli in the sputum was absolutely diagnostic of tubercle.

Riegel, of Giessen, and others, failed to find bacilli in the sputum of cases of diabetic phthisis. But I think the diabetes had nothing to do with keeping the bacilli out, as I have detected multitudes of bacilli in the sputum from a case of diabetic phthisis observed and confirmed by autopsy by Dr. Charles H. Reed, of this city.

Levinsky‡ and Koryanyi § both detected tubercle bacilli in the sputum of patients with syphilitic lesions of lung.

It is very probable that many of the cases of pulmonary disease in which bacilli were not discovered might nevertheless have been phthisical; in fact, the character of the control cases, as given by R. S. Smith quoted above, fully justifies such assumption. From the autopsy experience of clinicians and pathologists whom I consulted, and from observations of my own, I can testify that the only sure way to decide the nature of doubtful cases, such as, for instance, are recorded by Smith, is the autopsy; otherwise the negative evidence in relation to bacilli goes for naught. This is also substantiated by the observations of Gibbs, Whipham, and West, quoted above—viz., that bacilli may fail to appear in sputum where there are no cavities and no ulceration in the lung.

I have seen autopsies to reveal phthisis in cases where no bacilli were found during life, after careful examination over and over again repeated; and I also happened to witness the autopsies of three cases of non-tubercular lung disease which during life had been diagnosed as phthisis on account of bacilli found in the sputum.

* Cincinnati, 1883.

† Discussion before the Vienna Medical Society, Wiener Med. Presse, 1883.

‡ Deutsche Med. Wochenschr., No: 11, 1883. § London Med. Record, March 15, 1883.

The examination of sputum may thus, in doubtful cases, be quite misleading; for, if in any given case bacilli are not found, it should be taken in consideration, *first*, that the bacilli may be enclosed in the tubercle tissue, as in miliary tubercle, which rarely produces destruction of the lungs, and consequently may fail to appear in the sputum; and, *second*, that the examiner may fail occasionally in any case to succeed in preparing a successful preparation of stained bacilli. On the other hand, if bacilli are present, they sometimes may not be pertaining to the case, but be accidentally introduced through use of a vessel uncleansed and used by another patient, or otherwise; and, finally, it may be inferred, but it is by no means proved under rules of scientific scrutiny, that similar bacilli do not occur in the sputum of cases other than tubercular.

From our present knowledge of the occurrence of Koch's bacillus in sputum, we must therefore conclude:

1. That the presence of bacilli is a valuable *diagnostic sign* of tubercular disease of the lung.
2. That the quantity of bacilli found does not, as a rule, indicate the degree of the disease, and hence is *not a prognostic sign*.
3. That the absence of tubercle bacilli is *no proof whatsoever of the absence* of tubercular disease.
4. *The examination of air—viz., of the breath of patients* suffering with pulmonary tuberculosis, and of the air of sick-rooms and hospitals generally—has given some positive but no definite results.

C. Theodore Williams * “recently selected one of the ventilation shafts at the Brompton Hospital for consumptives, in which the flues of several wards converge, and in which extraction takes place at the rate of three hundred to four hundred feet a minute. In this current he suspended glass plates smeared with glycerine for a period of five days. The plates were then washed with distilled water, the fluid mixed with a little mucilage and evaporated down to half, and the residue tested for bacilli, which were found in fair abundance.”

R. Ch. Smith † “succeeded in demonstrating bacilli in the breath of consumptive patients by making them breathe through two thin sheets of gun-cotton placed in the outer compartment of an ordinary respirator. This layer of cotton is then converted

* Quoted after the *Lancet*, July 28, 1883.

† *Brit. Med. Journ.*, January 20, 1883.

into collodion, run in thin films on slides, and stained for bacilli."

A. Ransome* states that "on examining the breath of several advanced cases of phthisis, specimens of bacillus were found in two cases, while in several other cases the organism was not found, and it was not found in the aqueous vapor condensed in the waiting-room of the Manchester Consumption Hospital." The collections had been made by exposing cover-glasses smeared with fresh white of eggs or a little mucus for a certain length of time. Gibbs' method was used in staining.

Celli and Guarneiri† made similar examinations with quite different results. They were unable, after the most careful search, to find tubercle bacilli in the air of an unventilated room in which phthisical patients had been sleeping. The expired breath of those patients was likewise found to be entirely free from bacterial contamination. Nor could the tubercle bacilli be discovered in air which had been passed through the sputa of tuberculous patients, although in every case the expectorations were found to contain them in large numbers. (They were also unsuccessful in attempts at inoculation with fluids impregnated with this presumably vitiated atmosphere).

Prof. Sarmoni and Marchiafava (Annali Univ. di Med., Sept., 1883) examined the breath of a number of phthisical patients for bacilli, with absolutely negative results. They conclude that phthisis is not directly contagious, but might be indirectly so by means of dried powdered sputa which floats as dust in the air.

V. Wehde‡ made, under direction of Bollinger, in Munich, the following experiments in relation to examination of air. Plates smeared with glycerine were exposed for forty-eight hours in closed rooms in which there were a number of advanced acute cases of phthisis. No bacilli could be found after in appropriate manner applying the usual tests. He further testifies that after injecting the material collected, as above stated, into the peritoneal cavity of eleven rabbits and guinea-pigs, no tuberculosis was produced.

5. *Comparative studies of animal tuberculosis.*—Spontaneous animal tuberculosis is unquestionably identical with human tuberculosis. There are a few morphological specializations, which I

* Brit. Med. Journ., December 16, 1882.

† Quoted by the New York Record from the Gazzetta degli Ospitali, No. 56, 1883.

‡ Prager Med. Wochenschr., January, 1884.

mentioned in a former chapter—*e. g.*, in tuberculosis of birds and in bovine tuberculosis or pearl-disease; but the essential, peculiar histological features are the same in all. Tubercle bacilli appear also to be present in nearly all cases of spontaneous animal tuberculosis. I detected bacilli in a tuberculous bronchial lymph-gland from a phthisical tiger, which I had kept in alcohol for eight years; in one from a monkey of more recent date; and several times I found bacilli in spontaneous bovine, chicken, rabbit, and guinea-pig tuberculosis. I also studied tuberculosis in the bear, lion, leopard, and in a large variety of apes (dead of typical consumption, from the Zoological Garden of Philadelphia), with results identical with those obtained from studies in man. But this was long before the "outbreak" of the "bacillary campaign," and consequently Koch's parasite was not looked for in these latter cases.

Bollinger* found bacilli in the udder of a cow affected by pearl-disease (bovine tuberculosis).

There are no observations on record concerning the occurrence of tubercle bacilli in the excretions and the manure of animals affected by tuberculosis—sputum is not produced by animals—not even any reliable observation of bacilli in the milk.

Artificial or induced tuberculosis in animals will be considered in connection with the experiments further on.

6. *The occurrence of bacilli in lesions and substances other than tubercular.*—Bacilli not distinguishable from tubercle bacilli are met with in lupus and leprosy. The bacillus met with in lupus is unquestionably identical with the tubercle bacillus, as is evident from the investigations of Max Schüller, Pfeiffer,† Don-trelpont,‡ and Babès and Cornil.§ Yet the dermatologists are hardly inclined to recognize lupus and tubercle as inseparable, there being already a defined tuberculous lesion, the scrofuloderm, on the dermatological list; and, further, they refuse to identify the two lesions on clinical and anatomical grounds.

The bacillus of leprosy, in specimens which I had the opportunity to examine, appears to me also perfectly identical with the small forms of tubercle bacilli; although the lepra bacillus may perhaps look more sharp-pointed to the eyes of others and may

* Centralblatt f. d. Med. Wiss., August 18, 1883.

† Deutsche Med. Woch., No. 19, 1883.

‡ Monatsheft f. Praktische Dermatologie, No. 6, 1883.

§ Loc. cit.

fail to take the brown stain. There is nothing surprising in the fact that the same species of bacillus inhabiting soils of different character and different chemical composition, perhaps, may acquire varying micro-chemical properties and slight modification in shape. The experiments and evidence of Damsch,* Caposi † and Hansen ‡ further suggest the identity of leprosy and tubercle bacilli in their effects. There is no reason to believe that leprosy is a variety of tuberculosis, yet we must either declare lupus, leprosy and tubercle as identical lesions, or else declare the tubercle bacillus as not peculiar to tuberculosis.

I observed bacilli not distinguishable by the shape and micro-chemical tests from tubercle bacilli in the false membranes in two cases of diphtheria and in one case of scarlet fever with extensive pseudo-membranous angina. Two of these cases proved fatal; the autopsies did not reveal tuberculosis in any part of the body. The false membrane was prepared by crushing it between two cover-glasses, and treated like sputum.

Geo. Bodemar § discovered the tubercle bacillus in some of the lesions of typical cases of actinomycosis.

In syphilis of the lung the cheesy material and the sputum, as above stated, were found to contain tubercle bacilli by Levinsky, || and also by Koryanyi. ¶

Lichtheim ** and Craemer †† may also be mentioned in this connection as having each found the tubercle bacilli, or bacilli like them in every respect, in the fæces of a number of non-tuberculous patients, as well as in the tuberculous. This is, however, energetically contradicted by Gaffky, of Koch's laboratory, on the ground that he failed to discover in fæces of normal persons in Berlin any bacilli which reacted to micro-chemical tests like tubercle bacilli.

The discovery of Professor Balogh ‡‡ that bacilli similar to tubercle bacilli are found in the marshes around Pesth, Koch also tries to demolish by the statement that such bacilli were not detected in the mud of a Berlin city canal.

* Centralblatt f. d. Med. Wiss., July 21, 1883.

† Wiener Med. Woch., N. 2, 1883.

‡ Hospitals-Tidndee, No. 32, 1883.

§ Inaugural Thesis, Univ. of Pa., 1884.

|| Loc. cit.

¶ Loc. cit.

** Fortschritte der Med., vol. 1., 1883.

†† Sitzungsbericht der Societat in Erlangen, December 11, 1882.

‡‡ Wiener Med. Wochenschr., No. 51, 1882.

In sections of phthisical lungs I often observed masses of bacilli in those portions which were without tubercles, but which were affected secondarily by simple acute inflammatory changes and the air-vesicles merely stuffed with exudate undergoing rapid disintegration (coagulation necrosis); while the real tubercle tissue contained no bacilli, or sometimes only a few in the giant cells. I think Prudden* also noted this.

Surveying now the whole question of the habitat of the bacillus tuberculosis, it becomes evident that Koch's dogma—that and that only is tuberculosis, where his bacillus is found—is overdrawn and cannot bear criticism. It would be much safer to reverse this proposition, and to *consider that bacillus alone a tubercle bacillus which inhabits evident tubercular lesions or their products—e. g., sputum, and nothing else.* For we have no difficulty in diagnosing under the microscope a tubercle without the bacillus; but *a dilemma arises at once if we see questionable bacilli without the tubercle or outside of sputum.*

VI.—EXPERIMENTS AND EVIDENCE, PRO AND CONTRA.

It has been shown that the clinical evidence in reference to the contagiousness of phthisis is so meagre that assertions as to its parasitic origin are unwarranted (see chap. iv). Moreover, statistics negative such theory. This being the case, it would seem as if experimenters are trying to prove that which is not the reality.

The testimony of the defenders of this theory, however, appears strengthened since the publication of the discovery of the bacillus and of the experiments of Koch. This is to be in a measure explained by the impression which Koch's well-constructed article made upon the minds of some of our leading clinical teachers, who involuntarily felt themselves induced to teach and to write about the doctrine of the contagiousness of phthisis. The profession at large does not care for Koch's discovery, whatever its value may be; but the opinion of the leading clinicians endorsing such discovery forms a guide, and may prove one of the most efficacious means of influencing the profession in regard to the question of the contagiousness of phthisis.

Having arrived from my own experiments at conclusions different from those of Koch, I thought it at present timely to announce

* Loc. cit.

at least the results of my observations, as my detailed report cannot appear yet for some months to come. It is my personal observation, together with my conclusions obtained from a careful perusal of the control experiments and of the records of the observations of others, which determined my present attitude on the question of the etiology of tuberculosis.

The total evidence *pro* and *contra* gives me the impression that the doctrine of the contagious character and parasitic causation of tuberculosis cannot be sustained.

I will now submit a brief analysis and summary of experiments made and evidence offered in relation to the question of the parasitic origin and specific nature of tuberculosis.

For the establishment of a theory in regard to a parasitic origin of a disease by means of experiments on animals, etc., the following propositions must be affirmatively decided :

1. The disease produced experimentally in animals by means of inoculation with products of the human disease must be proven to be identical with the disease occurring spontaneously in man.
2. There should be some evidence showing that inoculation in man is followed by the same results as follow the inoculation of the same material in animals, and that the disease is really contagious.
3. There must be found a definite parasite at the beginning of the diseased process in all cases and in all tissues involved by the disease, and in sufficient quantity to account for the changes.
4. Given a parasite that is the cause of the disease, its action should be specific, *i. e.*, it alone should be the causative factor, and should, when isolated and inoculated into an animal liable to the disease, always produce that disease.
5. The lesions of a disease resulting from the inoculation of a specific parasite must also contain that parasite and the specific properties of reproducing the same disease when re-inoculated.
6. Finally, a given parasite and no other substance should, the conditions remaining the same, be capable of producing the disease.

Koch makes an effort to answer all the above propositions in the affirmative in reference to tuberculosis. As a thorough and experienced mycologist, he knew well that this is unavoidably necessary in order to establish the etiological relation of his bacillus to tuberculosis.

Tuberculosis was known before Koch to be inoculable, and was, upon popular notions and traditionally, known and regarded by some as a contagious disease. Taking such theory for granted, it was necessary to find the parasite. In fact, Klebs, Toussaint, Max Schüller and Aufrecht made excellent investigations, which even suggested the parasitic nature of tuberculosis, although the proofs offered by these investigators were not sufficient.

Koch's investigations, with his superior advantages, methods, and diligence, have been crowned with better success, and have brought forward facts of standing and permanent value to mycology, botany, and partly to medicine. His evidence in the question of the parasitic nature of tuberculosis is strong, but his conclusions from this evidence were overdrawn and too hasty. They are, so far, not as much justified as he and his followers think they are. There is great lack of that absolute proof that is necessary for the settlement of a question of such magnitude and social importance.

Koch has, in relation to tuberculosis, brought forward definite affirmative proof for only some of the above-stated propositions, and this, again, *only partial*. Valuable contributions to this end have been also made by others. But we must have *full proof for each and all* of those propositions, and these must be really applicable to tuberculosis, before we can accept the theory of a parasitic character and of the contagiousness of this disease.

Submitting, now, a brief criticism of the bacillus theory of Koch and his followers, I will take up separately each of the above-stated propositions, all of which it is necessary to prove in the affirmative before there is any reason for the establishment of such a theory in regard to tuberculosis.

1st. *The disease produced experimentally in animals by means of inoculation with products of human disease must be proven to be identical with the disease occurring spontaneously in man.*

In favor of the identity of human tuberculosis with that produced experimentally in animals there has been brought forward the fact that the products in both contain identical bacilli. But this surely does not prove the identity, because similar bacilli may be found in the lesions of various kinds of processes, resulting in cheesy products. (See bacillus chapter.) Besides, there are many spontaneous and artificially-induced tubercular lesions

in which bacilli could not be demonstrated. Hence we cannot rely upon the bacilli as a proof for the identity of the lesions.

Koch and those who imitated his experiments diagnose and declare all those artificially-induced lesions as tubercular which occur in nodes and in which they found the tubercle bacillus, without taking (as far as I know) into consideration any structural peculiarities or other conditions. Now, tubercle bacilli will surely be found in the lesions, whatever these may be, as they were introduced into the animal in those experiments. Further, in the opinion of these gentlemen nothing is tubercle where there are no tubercle bacilli. Therefore, how can we rely upon their statements as to what the lesions they induced in animals really were.

It is hardly reserved for the mycologist to teach us what is tubercle and what is not tubercle.

Tuberculous lesions with extensive cheesy changes and tissue destruction, cavities, etc., such as occurring spontaneously and often quite speedily in man or animals, cannot be induced experimentally by means of inoculation, unless very large quantities of some purulent tuberculous materials are used, and abscesses result. When an animal dies several or many months after the operation of natural tuberculosis, extensive caseation of the organs may occur.* The only kind of induced or artificial tuberculosis in animals which may be ascribed to the effects of inoculation is one that corresponds in naked eye appearance to secondary miliary eruption of tubercle as occurring in man—the acute miliary tuberculosis. This acute miliary tuberculosis in man, which I observed also in animals as a spontaneous disease, occurs only in wasting diseases accompanied by various grave symptoms, anæmia, and great emaciation; while the induced disease in animals occurs suddenly, does not induce any symptoms, no blood-changes, no emaciation, etc.

In many instances where the experimenters have produced, by means of tuberculous materials, within two to eight days after the operation, a miliary eruption, it is not probable that those miliary nodes were tubercles, and were due to the effects of bacilli, which grow extremely slowly, and it is not certain that the experimen-

* I was much surprised last summer to see in Berlin, at the Hygienic Exhibition, in Koch's pavilion, specimens of the character just stated exhibited as inoculation tuberculosis, and still more to hear the demonstrator explain (surely without being authorized by Koch) that these specimens were to demonstrate the rapid effects of the bacillus.

ters took pains to distinguish them from true tubercle, or were competent in all instances to do so. This is eminently true of the inhalation tuberculosis.

Tappeiner's induced inhalation tuberculosis of dogs,* so much relied upon by Koch and others for the establishment of the mode of the spreading of phthisis, and partly of the bacillus doctrine itself, has been proved to be a fiction. Tappeiner, as so often quoted, subjected dogs to an atmosphere heavily charged with phthisical sputum, so that the dogs were nearly bathed in the latter (known to contain bacilli) for weeks. But, in spite of this, the animals grew fat, if anything, and, after the lapse of a certain time, acquired local pulmonary affections in the form of nodules, not likely to have been tubercular in nature, of which only in one case some were observed in the liver and kidneys.

The experiments of Schottelius,† Wargunin and Rajewsky,‡ Weichselbaum,§ and of others,|| and my own experiments also (subsequently to be reported) make Tappeiner's assertions perfectly untenable. Tappeiner's own account of his experiments and the microscopical description of the structure of Tappeiner's "tubercles" by Grawitz and Friedlander in Virchow's institute clearly indicate that he had nodular broncho-pneumonic foci, and not tubercles. (See explanation of these formations in first chapter of this paper.)

I will, however, show later that pulmonary tuberculosis may occasionally be produced in rabbits by these means.

The following deserves a passing mention: According to Orth¶ and Bollinger,** there is some doubt as to the identity of human and animal tuberculosis. The results of the experiments of both these observers show that tuberculosis could only be induced by feeding animals with materials from animal tuberculosis; while tuberculous materials taken from man had no effect upon animals when given as food. On the other hand, the Wurzburg feeding experiments upon man †† prove that animal tuberculous materials have no effect on man.

* Virchow's Arch., vol. 74, 1878, and *ibid.*, vol. 82, 1880.

† Virchow's Arch., 73, 1878, and *ibid.*, 91, 1883.

‡ Vratsch., No. 6, 1882.

§ Centralbl., No. 19, 1882, and

|| To the same conclusion, I hope, will also come my esteemed friend Prof. Brose, if he repeats his experiments published in the Med. Record, January, 1884.

¶ Virchow's Archiv., vol. 76.

** Arch. f. Exper. Path., vol. 1.

†† Schottelius, *loc. cit.*

Although, judging from my own experiments, there is to my mind no doubt that some forms of artificially-induced tuberculosis in animals acquire gradually characters which make them identical with the spontaneous tuberculosis in man or beast, yet I do not think it at all proven that the lesions so rapidly arising from the effects of the inoculation with the bacillus of Koch are identical with tuberculosis in man. The proof, then, upon this point, the supreme one for the settlement of the question of the nature of tuberculosis, is yet to be furnished.

2d. *There should be some evidence showing that inoculation in man is followed by the same result as follows the inoculation of the same material in animals, and that the disease is really contagious.*

We have seen that clinical evidence and statistics do not elucidate a contagion for tuberculosis, and that the few isolated instances of apparent contagion offered cannot stand the test of scientific scrutiny. An infectious or contagious disease can have only one cause, and cannot be at one time due to a contagion and at other times arise from a variety of causes; hence the latter part of the proposition must be answered in the negative.

This being the case, the *parasitic* origin must also be denied it, as a necessary consequence.

As to the first part of the proposition, too little is known of scientific observation upon this point in regard to tuberculosis. According to the exhaustive investigation of Dr. Law,* there is no evidence that tuberculosis has ever been conveyed through vaccination. I must mention, though, an actual inoculation experiment upon man, not so much on account of its inherent value, but because it has been quoted with great reliance in support of infectiousness of tuberculosis. Demet Paraskeve and Zallonis, in Syria, Greece,† “inoculated a man of 55 with tubercle. He was suffering from gangrene of the left great toe, due to the obliteration of the femoral artery, to whom death was inevitable, as he had refused to submit to amputation. His lungs were carefully examined and found to be sound. They inoculated the upper portion of the right leg with sputa from a man who had abscesses in his lung. Three weeks later there were signs of commencing induration at the summit of the right lung. The

* National Board of Health Bulletin, No. 40, 1882.

† Quoted after Med.-Chir. Review, October, 1874, from Gazette Médicale, 1872, p. 192.

patient died on the thirty-eighth day after the inoculation from gangrene. At the necropsy there were found at the apex of the right lung seventeen small tubercles, varying in size from that of mustard-seed to that of a lentil. Two similar tubercles were found in the left apex, and two others in the liver. The experimenters concluded that the embryonic state of the tubercles and their limited number were due to the short time since the inoculation."

This isolated experiment, as well as any of the experiments on animals, is valid only when we take it for granted that the experimenters are able to differentiate spontaneous from artificially-induced tuberculosis. This is not probable in the case just quoted. We are told that the man experimented upon suffered from an exhausting disease, and it is well known that at least one-third of the autopsies in such cases reveal tubercular disease.

Directly bearing upon the proposition under consideration are again those Wurzburg feeding experiments, in which material known to be infested by tubercle bacilli was used often raw for years as food, under strictly scientific supervision, with absolutely negative results, and which tended to show that man does not react at all upon the tubercle bacillus.

3d. *There should be found a definite parasite at the beginning of the diseased process, and in sufficient quantity to account for the changes in all cases and in all tissues involved by the disease.*

In relation to tuberculosis this proposition cannot be answered in the affirmative; and it is by no means as definitely settled, as some high clinical authorities hold with Koch, that there is but one "specific parasite" in tuberculosis.

Klebs,* Toussaint and Schüller† have observed *micrococci* to be constantly present in tuberculous lesions and products (and have induced artificially the disease with the isolated micrococci), and no one has *proven* anything to the contrary; while Koch and Baumgarten‡ discovered *bacilli* in the same lesions. Koch claims for his bacillus more than is consistent with the laws of physiological and pathological life, and more than is in correspondence with the actual proofs offered in relation of the pathogenetic properties of this bacillus.

* Klebs now admits the bacilli, but denies that they are invariably present, and denies on ground of experiments their exclusive pathogenetic properties, although he admits that they are a not unimportant admixture to his micrococci.

† Loc. cit.

‡ Loc. cit.

The reports of some competent microscopists and pathologists (when the originals are examined) show that the tubercle bacillus is not invariably present in all cases and all products of tuberculosis; and, if present, it is often not seen in sufficient quantity to ascribe to it the claimed significance; and, furthermore, it is as a rule not present in the beginning of the disease. On ground of personal investigation I can offer similar testimony.

The bacilli should be present in every lesion, and in all cases and in the beginning of tubercular disease, and not chiefly in its degenerated products, if tuberculosis is to be called a parasitic disease, in accordance with the laws of pathology. In all well-established parasitic diseases the parasite is a necessary factor and is invariably present—unless there should be established for the “tubercle parasite” an exceptional, new, and mysterious mode of action.

The truth of the matter appears to be, and, indeed, from my daily observations in the laboratory upon a large quantity of material, I regard it as a fact, that the tubercle bacillus of Koch is a mere concomitant of cheesy disintegrated materials, even if it be pre-eminently of tuberculous cheesy materials.

4th. *Given a parasite that is the cause of a disease, it should, when isolated and inoculated into an animal liable to that disease, always reproduce that disease; but its action should be specific, i. e., it (the parasite) alone should be the causative factor.*

There is no doubt that Koch's tubercle bacillus, when isolated and cultivated for many generations and then inoculated into certain animals, is capable of inducing tuberculosis, or a nodular eruption not distinguishable from it, more readily than other irritants so far as tried. Success in inoculating is particularly frequent in rabbits and guinea-pigs (although not as common as Koch claims), but only conditional and rare in other animals.

Thus it appears that the above proposition could be answered for tuberculosis and the bacillus in the affirmative, if only the following points would be proven:—

1st. That the nodular lesion thus induced is really tuberculosis, identical with the human disease.

2d. That this bacillus is the only bacterium or the only irritant capable of inducing tuberculosis; and,

3d. That its action is specific, i. e., that the bacillus is the only agency or factor at work, the sole cause of the disease.

The first point is not proved, probable as it may appear. The other points are open to the following considerations and objections :—

It has been proved that in tuberculosis, micrococci as well as bacilli are causal, the evidence being "strong" for either "parasite;" whereas the bacillus alone should be the causal factor. As long as not *disproved*, Klebs', Toussaint's and Schüller's investigations (in relation to the micrococci as causal factors) have as much claim as Koch's. The method of cultivating those tubercle micrococci, as practiced by those investigators, was one not favorable for the development of the tubercle bacilli. Further, Watson Cheyne's assertion that bacilli *must have been* present in the cultured materials with which those investigators inoculated successfully is altogether a gratuitous assumption, and his few and imperfect control experiments with Toussaint's micrococci were not satisfactory, and, in fact, prove or disprove nothing.* Koch did not try the effects of any other fungus than that of his bacillus in relation to tuberculosis.

Koch further claims that the specific character of his bacillus is supported by the rapidity of its effects, and brings forward the inhalation experiments of Tappeiner and experiments upon the eye. The former I have shown to be valueless, as those nodules produced in the lung, especially if rapidly formed, are not tubercles. I have also reason to believe that the same is the case with many of the experiments on the eye, especially in those cases in which an apparently acute miliary tuberculosis of the lung rapidly followed the inoculation; in fact, in some instances this eruption occurred in a much shorter time than is at all possible for tubercles to develop.

Koch has not proved that his bacillus is the only agency at work in the production of tuberculosis. Although he undoubtedly inoculated the pure bacillus, he ignored the specific reaction of the soil; and it is the latter which I hold plays the most important rôle in determining the formation of tubercle. In introducing the bacillus into the animal organism, another factor, the injury inflicted, and its effects upon the living cells of the body, must be taken into consideration.

In some animals all the tissues of the body react equally upon the introduction of irritants; in others only some one of the

* See Watson Cheyne's report, *Practitioner*, April, 1883, pp. 272-276.

tissues responds, such as the serous membranes. This surely demonstrates the specific action of the soil.

I must again call attention to the fact that in making his experiments Koch injected the bacilli into any part indiscriminately in scrofulous animals, while in non-scrofulous animals (dogs, rats, cats) he injected them only into the peritoneum or anterior chamber of the eye, where, we know from experience and from repeated experiments, any irritant of sufficient intensity may create tuberculosis.

This cannot be explained (as is attempted by Koch) by the assumption that the bacillus must merely be enclosed in something so as not to be eliminated before it can exert its effect.

To me it appears that the reason why we must inoculate in serous cavities to produce tuberculosis in the dog or cat, is because we want not so much the specific action of the irritant (say of the bacilli) as the properties of the serous membranes. It is now well known that any chronic inflammation of serous membranes may lead to primary tuberculosis. It is proven that we do have a primary tubercular synovitis or a primary tuberculous pericarditis; and that bacilli could be instrumental in its production is highly improbable.

In surface tuberculosis like that of the lung the bacilli, in my opinion, also play only a secondary rôle.

Koch himself admits that it is not likely that the bacillus when inhaled by man could get a foothold in a normal lung. He says distinctly in his original articles that the lung must be pre-disposed for the reception and the action of the bacilli. Under such predisposition he understands and enumerates the following lesions: "*desquamation of the epithelial lining of the respiratory tract, stagnating exudates and secretions in the lung, adhesions, anomalies of respiration,*" etc. Now, here is a matter of mere interpretation of these lesions. Koch innocently calls them a "predisposition," while every pathologist will designate some of these lesions as suggesting already existing pulmonary phthisis. In fact, at the present standing of our knowledge of pulmonary phthisis we can have no desquamation of the vesicular epithelium without preceding tubercular infiltration.

Watson Cheyne is also considerate enough to say,* ". . . it seems to me that the lung must in addition be prepared for the

* Loc. cit., p. 314:

reception of the bacillus, as may be the case if congestion or slight inflammation be present at the time of the inhalation of the organism."

That in inoculations into serous cavities the latter do not act merely in preventing the bacillus to escape or to be eliminated, and that the stagnating secretions in the lung do not act merely as a glue to retain the bacillus in order to allow the accomplishment of its effects, is, to my mind, proven by the following experiments of Bollinger. Bollinger,* in order to show that tuberculosis could not be transplanted by vaccination, made superficial *cutaneous* inoculation in rabbits with tuberculous materials with negative, and deep *subcutaneous* inoculations with the same material, followed by intense inflammation, with positive results. In both cases the wounds were covered by a layer of collodion to prevent the "elimination" of the bacillus.

Thus it appears that the bacilli by themselves have no effect upon the healthy organism or the normal tissues. A predisposed soil is the chief factor and is pre-eminently necessary for the production of tuberculosis; while, on the other hand, it is not proven at all that the bacillus is invariably necessary for the production of tuberculous lesions. Although the tubercle bacillus is more liable to excite tuberculosis in an already inflamed and ill-nourished soil than all other simple irritants so far tested, it (the bacillus) might be readily substituted by other irritants.

The matter must unquestionably be tested further, but from the above evidence it is clear that a general fear of the bacillus tuberculosis as a contagion is unjustifiable, and that the ordinary dust suspended in the air is to certain persons as dangerous as the bacillus.

5th. *The specific lesions of a disease resulting from the inoculation of a specific parasite must also contain that parasite, and have the specific properties of reproducing the same disease when reinoculated in other animals.*

Koch claims that the products obtained in animals by inoculation with bacilli are capable of producing tuberculosis when inoculated into a second animal, while the products obtained by inoculation with innocuous substances do not have this effect. The former proposition is true, but the latter, I hold, is not in accordance with facts. In my own experiments, to be detailed

* Zür Aetiologie der Tuberculose, Prager Med. Wochenschr. N. 4 and 5, 1884.

in my forthcoming report, tubercles produced by inoculation with innocuous material under antiseptic precautions were likewise capable of producing tubercles when inoculated into other animals, having thus the same action as the innocuous material primarily used.

I have also shown above (see bacillus chapter) that in secondary tuberculous products bacilli may be absent.

The experiments of Martin,* which tend to show even the progressive virulence of products obtained from reinoculation with tuberculous material in a series of animals, have been substantiated by no one.

Martin's assertion also, that inoculations with products obtained by the introduction of innocuous substances never produce true tuberculosis, and that after a series of reinoculations these products lose their power of acting even as local irritants, is, according to control experiments, positively wrong. On the other hand, views have been expressed, based upon experiments (I think also by Martin), that products obtained by inoculation with non-tuberculous substances when reinoculated may gradually become specific, and increase in virulence in producing tuberculosis.

6th. *Finally, a given parasite and no other substance should, the conditions remaining the same, be capable of producing a parasitic disease.*

In my previous studies, judging from the literature alone, I was fully impressed with the idea that tuberculosis had a specific exciting cause, and that it could be induced by inoculation with tuberculous materials. Moreover, having made numerous inoculations with tuberculous matters, I convinced myself of this fact. Hence I accepted the view that tuberculosis is inoculable in certain animals.

But, at the same time, after repeating, under various modifications, the well-known control experiments, I found that, beyond doubt, even true tuberculosis could be induced by substances other than tubercular, and that failures to induce tuberculosis with tuberculous materials were in certain animals nearly as common as inoculations with innocuous substances.

To these experiments I will return in my forthcoming report.

It will be also necessary to first consider the evidence of those observers who, from the results of their own exhaustive experi-

* Journal d'Anatomie et Physiologie, April, 1881.

ments, negated the exclusive or specific infectious properties of tuberculous materials. This negative evidence is by far more voluminous and stronger than the admirers of the hypothesis of the contagiousness of tuberculosis suppose; excited admirers having especially arisen since the ingenious article of Koch appeared.

It is, however, remarkable that some of the writers on tuberculosis fail to understand that the pivot of the question of the etiology of tuberculosis does not rest upon the fact alone whether or not the bacillus induces lesions analogous to tuberculosis, but pre-eminently upon the fact whether innocuous substances have or have not the same effects.

Thus, above all, the negative evidence must be carefully inquired into, not by relying upon the crippled and sometimes misrepresenting and meagre quotations of some of the compiling writers, but by submitting the original communications of the authors and experimenters to a careful perusal.

Together with the accounts of the much-quoted experiments of investigators who succeeded in inducing tuberculosis in animals with tuberculous substances only, the reading and thorough examination of the records and results of experiments of the observers to be mentioned below are unavoidably necessary.

The following observers all refer to many or few experiments of their own in which tuberculosis resulted from the inoculation with either innocuous substances or with specific matters other than tuberculous :

- Lebert, *Allgem. Med. Central-Zeitung*, 1866.
- Lebert and Wyss, *Virchow's Archiv*, vol. xl, 1867.
- Empis, *Report of the Paris Internat. Med. Congress*; 1867.
- Burdon Sanderson, *British Med. Journal*, 1868.
- Wilson Fox, *British Med. Journal*, 1868.
- Langhans, *Habilitationschrift*, Marburg, 1867.
- Clark, *The Medical Times*, 1867.
- Waldenburg, *Die Tuberculose*, etc., Berlin, 1869.
- Papillon, Nicol. and Leveran, *Gaz des Hop*, 1871.
- Bernhardt, *Deutsch. Arch. f. Klin. Med.*, 1869.
- Gerlach, *Virchow's Archiv*, vol. li, 1870.
- Foulis, *Glasgow Med. Journal*, 1875.
- Perls, *Allgemeine Pathologie*, 1877.
- Grohe, *Berliner Klin. Wochenschr.*, No. 1, 1870.
- Cohnheim and Frankel, *Virchow's Archiv*, vol. xlv, 1869.
- Knauff, *41te Versamml. Deutsch. Naturforscher*, Frankfurt.

- Ins, *Arch. f. Experim. Pathologie*, vol. v, 1876.
 Wolff, *Virchow's Archiv*, vol. lxvii, 1867.
 Ruppert, *Virchow's Archiv*, vol. lxxii, 1878.
 Schottelius, *Virchow's Archiv*, vol. lxxiii, 1878; *ibid.*, xci, 1883.
 Virchow, *Virchow's Archiv*, vol. lxxxii, 1880.
 Stricker, *Vorlesungen über Exp. Pathologie*, Wien, 1879.
 Martin, *Med. Centralblatt*, 1880, No. 42.
 Wood and Formad, *National Board of Health Bulletin*, Supplement No. 7, 1880.
 Robinson, *Philadelphia Med. Times*, 1881.
 Weichselbaum, *Med. Centralblatt*, No. 19, 1882, and *Med. Jahrbucher*, 1883.
 Balough, *Weiner Mediz. Blatter*, No. 49, 1882.
 Wargunin, *Allg. Med. Centralblatt*, April 8, 1882.
 Hansell, * *Arch. f. Ophthalmologie*, vol. xxv.

Some of the observers enumerated did not consider the miliary eruptions obtained experimentally as true tubercles, but the majority did so, and, as I will show later, presented excellent and reliable experiments and sound reasoning in support of their views.

Shall all the above evidence go for naught merely because Koch has discovered a bacillus which is capable of inducing in animals lesions resembling tuberculosis?

I trust it will not. Koch has, so far, no authority to claim *exclusive* pathogenetic properties for his bacillus, as he made himself no satisfactory control experiments with substances other than tuberculous. The few control experiments he offers, viz., that *sterilized* blood-serum (1), tuberculous material soaked in alcohol, and fresh scrofulous glands, or pus from tuberculous lesions, did not induce tuberculosis, prove very little or nothing in favor of his bacillus.

* Hansell, who inoculated animals with gummous growths and syphilitic pus, obtained an exquisite miliary tuberculosis from the effect of these substances. In this connection may be yet mentioned:

Damseh (*Centralbl. f. Med. Wissen.*, July 21, 1883) obtained tubercular eruptions and nodes in the brain in rabbits through inoculation into the eye with the cultivated bacilli of leprosy. Similar inoculation with leprosy material led to a perfect miliary tuberculosis in rabbits in the hands of Kaposi, of Vienna (*Wiener Med. Presse*, January 21, 1883).

Pfeiffer, Dontrempont, Cornil, and Babes (*loc cit.*) had the same experience with lupous material.

Bodamer (*Inaugural Thesis*, Univ. of Penn., 1884) had, as the result of inoculating with the pure cultivated actinomyces fungus, a striking general miliary tuberculosis in rabbits.

Inoculation with materials from glanders gives also rise to tubercles in the lungs, etc., not distinguishable under the microscope from true miliary tuberculosis. But Löffler, who kindly demonstrated to me this fact in Koch's laboratory, and who also gave me a specimen demonstrating it, explained that the nodules in the lungs were not tubercle, because the bacilli found therein behave differently in staining.

Watson Cheyne, in his excellent report,* developed great care, diligence and skill in his experiments and observations intended to corroborate Koch, but in making his control experiments he likewise was not very particular. So in relation to inoculations with non-tuberculous substances he came to the conclusion that "not one of the twenty animals (inoculated with innocuous substances) became tuberculous!" But when the detailed account of Watson Cheyne's experiments is read over, it is amusing to learn that only nine out of the twenty-five supposed negative experiments were really known to be negative, because eleven of the rabbits experimented upon had been stolen before Cheyne had a chance to examine them, two rabbits died within a few days, or long before tubercle could develop, and in three rabbits the experimenter really records lesions that might have been tuberculous, in spite of the absence of bacilli in them, which latter circumstance, however, induced him to call the result a negative one.

These are instances how experimenters with preconceived and peculiar ideas upon a subject may unknowingly be misled in forming conclusions from their own experiments.

Further, it is interesting to note that in the "classical" experiments of Solomonson,† Baumgarten,‡ Tappeiner,§ etc., among other substances, the following materials were used extensively for control: "Caseous glands from scrofulous child," "caseous material from various sources," "muscle, testicle and kidney from tuberculous guinea-pig," "cheesy pus from man and animals, cheesy infarcts, caseous tumors," etc. All these substances, which are known usually to contain the bacillus, were inoculated while fresh into animals, and are recorded by the experimenters above stated to have failed in producing tuberculosis. This is indeed not consistent with the doctrine of Koch.

Wherever inoculation with innocuous substances was followed by positive results—the over-zealous germ-theorists call it "*accidental tuberculosis*." They say that at the time of former experiments the communicability of tubercle by a mediate contagion was not recognized, and as the precautions necessary for thorough disinfection of instruments, surroundings, etc., were probably not

* Loc. cit.

† Aftryk fra Nord Med. Arkiv, vol. xi, 1879.

‡ Loc. cit.

§ Loc. cit.

observed, the channels for the introduction of the *bacillus* were in all previous experiments left unguarded; hence they argue *it must have been* this ubiquitous bacillus which induced the tubercle.*

Further admitting, however, that innocuous substances may induce tubercle-like bodies, they claim that these bodies are not infectious, *i. e.* they are *false* tubercles.

All these objections would be very plausible if they were based upon actual observations and facts; but, unfortunately for the bacillus theory, they are not—they are *mere unfounded assumptions*.

The fact established by experiments, that a true tuberculosis can be induced in animals by inoculation with innocuous and various other substances, and the significance of this fact, can surely not be overthrown by the imperfect evidence that the bacillus is more liable to do so, and still less by mere unauthorized opinions of some of the writers.

Erroneous conclusions and views may easily be formed through the misconception of the significance of experiments.

In the meeting of the Pathological Society of London (Dec. 4, 1883, quoted from the *Lancet*, Dec. 8, 1883), Dr. Wilson Fox announced the following: "He was unwilling that his former observations (*loc. cit.*) should still be quoted as opposed to the doctrines of Koch and those who had been more recently work-

* In this connection the following incident is interesting, particularly on account of the high authority of the observer:

Some experiments were made under the supervision of Virchow (*Berlin. Klin. Woch.*, 1880), principally with the view of testing whether the milk of animals affected with "pearl disease" or bovine tuberculosis could reproduce the disease when fed to other animals. Virchow's own objection to experiments of this kind is that the various chronic inflammatory processes, which occur spontaneously in animals, are not sufficiently well known, even to veterinary specialists. Further, in pigs, which he used in considerable numbers, from their alliance to man, through their omnivorous habits, scrofulous glands occur so frequently, and their detection during life is a matter of so great difficulty, that results founded upon their presence must be accepted with great caution. The possibility of coincidence was also well illustrated by two cases, in which several animals were found to be tuberculous after having taken the milk for some time from a cow which was diagnosed during life as affected with bovine tuberculosis, but whose lungs were found, at the autopsy, filled with echinococcus cysts, and with no trace of tuberculosis.

The milk of another animal, which subsequently was found to be profusely affected by bovine tuberculosis, had, on the other hand, no effect when given as food to healthy animals.

The only result that Virchow thinks is perhaps justified from these experiments, is that more animals were found to be tuberculous among a certain number which had been fed upon the "pearly" milk than among the same number which had been fed upon healthy milk.

(The above statements [first quoted in a paper by Dr. Whitney] Professor Virchow corroborated in a conversation with me upon this subject last summer.)

ing at the subject; and therefore he had felt bound to come forward and make known the modification which his views had undergone." At the same time Dr. Fox, however, added, "there was perhaps some danger of phthisiophobia or phthisiomania. During the past thirty years there had been many changes in the doctrine of phthisis, and hardly any doctrine has lasted more than five years."

But what had happened to induce Dr. Fox to lose faith in his own honest work? From what I could learn so far it was the following: Dr. Fox had requested a Dr. Dawson Williams to repeat his former experiments. This bacillus-excited gentleman introduced *carefully* some "*putrid fluids*" and some *setons* into a few guinea-pigs and—did not obtain tuberculosis! Now, they think, it was at once evident that in all the former successful inoculations with non-tuberculous materials the mischievous bacillus of Koch must have gained entrance.

The reasoning of the London gentlemen appears to have been here as follows: *Putrid matter and setons do not induce tuberculosis; but the bacillus does. Hence, the bacillus is the sole specific cause (?)*.

But what is gained or proven for the bacillus theory if any one given substance, when inoculated into an animal, does not induce tuberculosis? Does, through this, the necessity of contagion at once arise? Surely not. If, for instance, as I will prove, finely-powdered, sterilized glass is capable of inducing a true tuberculosis, then it does not matter if putrid matter or setons failed to do it.

Cohnheim's acceptance of a theory of a specific poison for tuberculosis, which formed the basis for its direct outgrowth, the bacillus boom, was also not justified from Cohnheim's own experiments. If he once succeeded * with innocuous substances in producing *peritoneal* tuberculosis, it is of no consequence even if he subsequently failed to induce an *iris tuberculosis*.

Negative results prove nothing under the above circumstances and in the presence of positive results. Most of the observations made in bacillus studies prove really nothing for the etiology of tuberculosis, and some interpretations of the results of experiments in this direction are quite deficient and not consistent with the principles of experimental pathology. Furthermore, some of

* Loc. cit.

the positive evidence must be excluded on account of the evident deficient knowledge of pathological anatomy on the part of some of the experimenters.

I am glad to be in the position to offer in my next communication a series of observations and experiments on tuberculosis. These experiments, instituted under the auspices of Dr. Pepper, Provost of the University, and executed by myself and assistants under all rules of scientific precautions and under full facilities for such work, plainly demonstrate that *the etiology of tuberculosis does not rest with Koch's "parasitic" bacillus or any other "contagion."*

The experiments referred to will be given in full details in a special report now in progress and soon to be published with appropriate illustrations, etc.

I desire, however, to announce here that *my experiments prove that finely-powdered, sterilized glass, ultramarine blue, and other substances are by themselves capable of producing tuberculosis in animals or tissues liable to this affection.*

Further, I will offer proof that this effect (tuberculosis) ensues without the intercurrent action of any bacterium. And, finally, that in those instances where miliary, nodular eruptions have been induced by the tubercle bacillus (or substances containing it), the action of the latter is a purely mechanical one, like that of simple irritants.

Further, these experiments show that the only advantage which the bacilli have over other finely-divided matter and simple irritants is that the former multiply and thus intensify their action, while mechanical irritants have not this property, and hence must be introduced in larger quantities. The more finely divided the matter is, the more prompt seems to be its effect, and I believe it is impossible to render any matter more finely divided than the bacilli.

Like others, I also often succeeded in tracing the formation of the tubercle nodules to the effects of the irritating particulate matter, if the latter were or could be made distinct enough to be seen within the nodes. If ultramarine blue was used for inoculation, granules of the latter substance were seen within the nodes; if bacilli were used to that end, then bacilli could be detected within the nodes. But in either case these primary

nodular eruptions, *if rapidly formed*, do not yet represent tuberculosis, as I will show.

It is generally conceived that a specific infectious disease, such as instanced by variola, syphilis, anthrax, etc., can have only one cause or one poison which will produce that disease and nothing else, and cannot be substituted by anything else.

For tuberculosis this is not true, for we have bacillary and non-bacillary forms of tuberculosis.

It is now no more a question of observation and experimentation, but rather one of interpretation and understanding of the results; for we have seen that the evidence from experiments and microscopical studies is nearly sufficient.

But there are misconceptions. If that only is tuberculosis where the bacillus of Koch is found, or that only which arises from the effects of this bacillus, then Koch's theory of the exclusive pathogenetic properties of the bacillus is correct, and under such a definition tuberculosis has only one cause. But if true tubercles exist and can be produced without the bacillus, which has been shown to be the fact, then Koch's theory cannot be accepted from a pathologico-anatomical standpoint, or else we are obliged to admit two or more kinds of tuberculosis—one due to Koch's parasite, and others to a variety of causes.*

So far, however, we have no reason, from a pathologico-anatomical standpoint, to subdivide tuberculosis, and therefore I am of the opinion that Koch's view of the exclusive pathogenetic property of his tubercle bacillus is decidedly overdrawn and even not warranted by facts. Neither the specific action of Koch's bacillus, nor the specific character of tubercle, nor the contagiousness, is proven.

Only after a complete harmony of the facts derived from pathologico-anatomical, experimental, and clinical studies in tuberculosis, with those revealed by mycology, and not from either of these alone, can we arrive at a settlement of the question of the etiology of tuberculosis.

Further details concerning this question will be incorporated

* A suggestion to separate an "infective" form of phthisis from ordinary phthisis has been made by Dr. Reginald Thompson (*London Lancet*, No. 6, 1880, quoted after R. S. Smith, Bristol, *Medico-Chir. Journal*, No. 1, 1883). "In a series of fifteen thousand cases observed, fifteen cases (only one per one thousand) proved to be of an infective kind, viz., with history of contagion and absence of phthisical family history"

in my report. This will embrace also studies into the onset and the distribution of tuberculous affections.

From the above analysis of the bacillus question and of the etiology of tuberculosis the conclusions follow:—

1. That the bacillus of Koch is a valuable diagnostic sign of tubercular disease.
 2. That nothing is proved by its discovery for the etiology of tuberculosis.
 3. That the too ready acceptance of the bacillus doctrine is not justifiable, and is likely to do more harm than good.
 4. That neither phthisis nor any form of tuberculosis is contagious.
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INFLAMMATION OF THE EAR, AND ITS RELATIONS TO WHAT IS COMMONLY CALLED "TAKING COLD."

Read February 27, 1884.

BY BENJ. J. RUDDEROW, M. D.

IN presenting my paper this evening, it is not as an expert nor to specialists in this branch of medicine that I wish to speak, but to place before the general practitioner some few points that may help prevent some of the bad results which are so frequently met with as come from the tardy, timid, or ignorant management of such ear affections as arise from an ordinary catarrh, or that happens as one of the complications of sore throat, measles or scarlet fever. It is from the so-called taking cold that we find many of the acute or middle-ear diseases arising. When one is attacked with a cold, or has a more or less acute catarrh, it is generally traceable to a sudden or unexpected fall in temperature, or the exposing of a limited portion of the body to moisture, or it may be to the cooling effect of air in motion, or the depressing effect of air overheated or impure in assembly rooms, dormitories, etc. It often happens that the best conditions of ventilation and heat cannot be had; then how may the body be prepared to not only withstand the inevitable exposure, but to make a draught of air one of pleasure and of health?

Living as we ordinarily do, without a sufficient amount of active exercise out of doors, the surface of our bodies becomes morbidly sensitive to those influences that produce an acute catarrh. Thus one finds amongst people great dread of a draught, and the average

person will rather incur the risk of contracting a fever or some contagious disease in a crowded vehicle or assembly, than that he should have these places freely ventilated. By far the greater majority of mankind, while resting, consider air in motion to be one of the most morbid influences that one may meet. It is this, I think, that presents us many obstacles in the way of properly ventilating public vehicles and buildings. We do not, I think, find many persons so educated as to endure, when at rest, air in motion, far less to find pleasure and health from it.

But I speak from experience when I say it is very easy to overcome this morbid sensitiveness and often fatal tendency to become an easy victim to the causes of a common cold. But how may one teach himself to endure a draught and lessen the tendency to catch cold? By first diminishing the morbid sensibility of the body to catch cold, and this can best be brought about by a graduated exposure and friction of the skin in a daily air- or sun-bath, to be followed by such local sponge-baths as one may be able to react from speedily. This reaction to be speedy and spontaneous, it is necessary that the temperature of the water should not be much lower than 80° Fahr.

With the first air-bath it is well that the body should be exposed but for a short time only, as, for instance, the time taken to walk briskly across an ordinary bed-chamber. Soon by a little practice in this way, the time of exposure may be prolonged to fifteen or twenty minutes, and the temperature of the bath used accommodated to that of the outer air. The healthful effects of these exposures may be still further increased by two or three deep, chest-filling inspirations, with closed mouth, and by a few such movements of the arms as would tend to invigorate the chest-muscles and quicken somewhat the action of the heart. About four times a week, before any water is applied, and while taking the air-bath, the whole surface of the body should be briskly rubbed till there is a sense of glow and warmth of the skin, with hair-mitten, flesh-brush or coarse towel. These exercises should at first be of short duration, and more especially so if the heat-producing powers of the body are low. Also, if the subject be not too feeble, the rubbing should be done by himself rather than by another, as it is thus made more beneficial. These exercises should, in all cases, not be attempted in winter, except in a sunny room or one heated artificially.

We should bear in mind that the object of our treatment is to make the body less morbidly sensible by the exposing of the entire skin surface daily to air, light friction and cleansing, in an atmosphere nearly like the prevailing temperature. These daily air-baths should not be indulged in for too long a time, nor yet should invalids indulge in the full practice at once, but enter upon it deliberately and cautiously, as one not knowing how to swim enters the water gradually. These few hints, with the cautions given, will, in a few weeks, make almost any one not only less susceptible of taking cold, but better in every way; but at the same time I do not wish it to be understood that by training the body to be less susceptible to the sudden changes in temperature, that one may with impunity, without hat or coat, stand exposed to a northeaster.

In the preventive treatment of catarrhal affections of the middle ear, food, clothing and exercise are hygienic factors that have important relations in the causation of catching cold. Alcoholic stimulants, even in moderate doses, do increase the liability of taking a cold, and one who has been deluded by the idea that by so doing he can keep out cold, had best immediately put on an additional covering to keep his animal heat in. As a rule, except in certain conditions, alcohol is a very bad food, and what those conditions are has not as yet been determined with anything approaching an agreement by even the most learned chemists and physiologists. Articles of food that help produce dyspepsia, such as greasy fries, hashes, and other messes found on the tea and supper table, so fearfully and wonderfully made, should be avoided. Train the stomach and digestive organs to be the servant, not the master; keep them in vigorous action by the use of coarse farinaceous foods and milk. The farinaceous articles of food contain not only many tissue-building ingredients that are indispensable, but in a mechanical way, by their contact with the digestive organs, do a work similar to that which friction of the skin does for the surface of the body in hastening the desquamation of effete and sticky epithelium and the cleansing of the follicles.

Let the breakfast be one of thoroughly boiled farinaceous food, eggs, milk or cream and fruit; a light lunch of fruit, or milk and bread; dinner hearty, but free from condiments and spices, including fish, meat and vegetables; the tea somewhat similar to the breakfast. As to clothing, flannel should be worn next the skin, day

and night, by persons of all ages. The notion that one can toughen children by insufficient clothing and a promiscuous diet, is a fallacy, as we all know.

We often meet people, otherwise intelligent, who keep the legs and chest of their children bare, and go themselves without flannels from the supposed good health of savages and paupers. The flannels worn by day should be changed at night, and when taken off should be turned inside out and allowed to dry and air thoroughly. The neck-ties and bands should be loose, so as not to check the return circulation in the neck; the foot-covering thick, loose, and low-heeled, with a broad sole, allowing the interosseous spaces room, so as not to press on the nerves and vessels that vivify the toes.

From the preventive measures, we will pass to some of those conditions which demand promptness on the part of the medical practitioner in his effort to cure the external or middle ear inflammation at the earliest moment. It is not necessary to systematically go over the various diseases, my purpose being to give a few points that may be of use to the general practitioner, and not my views or opinions as an expert.

Every medical man should be able to distinguish the drum-head or tympanic membrane, and any one who can introduce a catheter can incise a drum-head. Ear-ache, generally badly treated, arises commonly in the beginning from one or two forms of ear disease, either inflammation of the dermoid or periosteal lining of the external auditory canal, or an acute inflammation of the middle ear. Pain and deafness often occur in the course of common boils in the external auditory canal. How is one to make the distinction between such a disease or something deeper seated. Each one should make himself so familiar with the external auditory canal as to tell at sight, whether a given canal is changed in calibre or not, and if changed what is the nature of the change. Inspect the canal, and with a probe, guarded with a piece of cotton, explore it thoroughly, carrying the probe around its entire circumference, touching each segment. In this way one can ascertain whether there is a focus of local inflammation, and having found it, then with a sharp, curved bistoury, incise clear down to the bone. The earlier this is done the better. After the incision is made, foment the ear with warm water from a fountain syringe; poultices in these cases to be avoided, as the uninterrupted appli-

cation of heat tends to beget oedema, making the canal more or less boggy, thus helping to produce a successive crop of boils, or setting up an obstinate or diffuse inflammation. If incision should do no good, apply one or two leeches in the hollow at the base of the tragus, half an inch in on the front wall of the external auditory canal. Two or three leeches applied in the position mentioned I have found to do more good than when applied in front of the tragus, or over the mastoid, except there be an inflammation of the mastoid cells, commencing to outcrop behind the external ear.

Persons consult us in reference to pain and deafness in the ear; they tell us that they have had a cold, a sore throat, or have been using a nasal douche; their rest has been broken, they have had continual pain and agony; what are we to do—order a poultice, and give an anodyne to relieve the pain, or, if in the case of a child, tell the parents to apply a poultice and await the discharge of pus? It is just such cases treated in this way that go to make up a large bulk of incurable cases of otorrhœa; they are the ones that worry the aurist by their obstinate character, and end in fatal temporal or bone disease. When consulting a patient suffering as above, act promptly. Take the hearing distance with the watch, examine the auditory canal and see that there is no inflammation there external to the drum-head. Apply leeches inside the hollow of the tragus, favor bleeding by hot fomentations, and if the pain and deafness are not relieved within a few hours, incise the drum-head with a fine straight knife or bistoury. Carry the incision from just below the extremity of the long process of the malleus to the lower border of the membrane. After this has been done, inflate the ear with a Politzer inflator, or get the patient to hold his nostrils closed by means of his fingers, and then blow strongly with closed mouth into the nostrils.

Syringe the ears with a warm solution of salt and water, carbonate of soda, or boracic acid. This syringing with warm water is objected to by many. After the syringing give your anodynes; not before, as the relief given before the incision may mask the processes going on in the ear, and stupefy your patient until something bursts, that meaning generally a more or less hopeless rent in the membrane, or, this not happening, a thorough invasion of the mastoid cells. Repeat the leeches if necessary and the paracentesis to cut short the inflammation. Though the para-

centesis should be done every day for a week or more, one need not be afraid, I think, of injuring it, as it will not do as much harm as to leave the products of inflammation dammed up in the drum-cavity, to destroy by their macerating process the machinery, threaten the portals of the internal ear, stuff the mastoid cells, or break through a more or less disorganized ear-drum. As a rule, persons suffering with ear-ache need not be kept indoors except for a few hours, unless in bad weather. Moderate walking appears to lessen the pain, and seems, whether from the posture of the body or the influence of that form of locomotion upon the circulation in the head, to quicken the healthy process of resolution. Give the anodynes when the pain is severe, and encourage the sufferer to walk about slowly until a decided sleepiness is induced. But the principal object in this stage of the disease is to keep a free opening through an artificial drum opening, in order to allow of the escape of all inflammatory products. See the patient every five or six hours during the first two or three days, and whenever it appears that the opening has closed that has been previously made, repeat the paracentesis. If the opening previously made should be closed, if possible, pass your knife through it; but if that cannot be done, then make an incision through the lower part of the drum, below the malleus.

Morphia, chloral and bromide of potassium to relieve pain, often a large dose of bromide, with a hypodermic, will produce happy results. Some form of magnesia salts is, I think, an admirable laxative, or the bitter water alone, or with hot water, a gill or two of the former to a pint of the latter. Let the diet be nutritive, especially in the early part of the day, but as the pain occurs mostly at night, it is best not to fill the stomach at that time.

One cannot be too prompt on evacuating the middle ear when there is an inflammation of that part. Delay is dangerous, and the paracentesis does no harm.

In measles and scarlet fever, examine the ears daily, and anticipate, if possible, the ulceration of a tympanic membrane, which so frequently occurs in the progress of these diseases. If the general practitioner would only examine daily the ears of those afflicted with scarlet fever or measles, and, finding them inflamed, treat them promptly, a great number of those obstinate or in-

curable otorrhœas and middle-ear troubles met with would be prevented.

No practitioner is really prepared to treat a case of scarlet fever or measles who is not able, at least, to recognize the drum-head when he sees it, and to perform the simple operation of paracentesis.

DISCUSSION ON INFLAMMATION OF THE EAR.

DR. C. H. BURNETT, in opening the discussion by request of the Chair, said: It is very important to find out the real cause of the pain and inflammation in the ear. Very often the pain is due simply to a closure of the Eustachian tube and not to inflammation in the tympanic cavity, or it may be due to an inflammation in the membrana flaccida, or Shrapnell's membrane, in the upper part of the membrana tympani, the latter being unaffected. In the former instance the membrana tympani is drawn inward by the vacuum formed in the middle ear by the closure of the Eustachian tube, the ossicles are forced inward, pressure is exerted on the contents of the labyrinth, the filaments of the auditory nerve in the semicircular canals are compressed, and reflex irritation of the cerebellum, with vertiginous symptoms, result.

Inflation will remove in many cases the pain in the ear and other symptoms, without the resort to leeches or paracentesis.

The surgeon should not be swift to incise the membrana tympani, especially simply to relieve pain in the ear. The membrane should be incised only when it bulges in consequence of fluid accumulations within the tympanic cavity. If an acute inflammation exists in the membrana flaccida, causing pain, this part of the membrana tympani may be incised with great advantage. Repeated incisions in the membrana tympani, especially in acute cases, for the relief of pain, are rarely if ever demanded. When suppuration and discharge have been fully established, cleansing by the use of absorbent cotton on the cotton-holder and the use of powdered boric acid, either pure or in combination with resorcin or chinoline salicylate, *i. e.*, the dry treatment, will be found more efficient than the use of the syringe or astringent drops.

Recurring again to acute cases of inflammation in the ear and ear-ache in children, I would recommend a sudorific and anodyne treatment with the administration of aconite in proper doses, as preferable to depletion or scarification about the ear. The child should be of course carefully housed during the acute and painful stages of the disease. The surgeon should bear in mind that when called to see a child or any one affected with an acute inflammation in the ear, that domestic remedies may have been used, and have aggravated and masked the disease. Very often the ear pain may be relieved by simply washing from the auditory canal various irritant salves, and a host of pungent domestic remedies, which would be enough to make a well ear ache.

Dry heat is the best remedy in the early stages of many cases of otitis, and its application will often bring about a resolution of acute congestion in children or adults. In fact, at the outset mild remedies or nothing should be applied to an ear acutely inflamed, whether the seat of the disease be in the external canal, the drum-membrane or the drum-cavity.

If a furuncle be diagnosed in the auditory canal, it should be incised at once. This usually obviates the necessity of all forms of poulticing, and prevents the tendency to the formation of other boils by cutting short the congestion in the skin of the canal.

DR. LAWRENCE TURNBULL: I cannot but express my pleasure at the way the subject has been brought before us, namely, the hygienic importance of the subject. Most of you are aware of the publication of my little work on this subject, and of the proposition which will be found in it worded as follows: "Of all the injurious influences combined, none, however, are so hurtful to the integrity of the human ear as cold."

All the main propositions which the gentleman has put forth in his essay I approve of, but I would make this reservation, that it is always a difficult matter to determine whether we have a true catarrhal inflammation of the middle ear to treat, or a purulent one, so frequently do the symptoms run together and become mixed. In the ordinary or simple form of catarrhal inflammation of the middle ear, you will have a rapid development of acute inflammation with pain, heat, swelling and redness, and in from twenty-four to forty-eight hours a sudden giving way of the drum-membrane, followed by a discharge of mucus not mixed with fully-developed pus, and the child almost soon recovers from it.

In the second form you have an acute inflammation which involves the deep-seated tissues, blood-vessels, etc., and if not checked promptly will ultimately destroy the membrana tympani, and may involve the bone or brain itself, with profuse discharge, for days, weeks, or even months, of pus, blood, and broken-down tissue and bone. Now, I consider that the most important treatment is to relieve pain, and I find in little children with their distressing cries which appeal to every one's heart, that nothing is so good in the early stage as to apply a bag of hot salt or sand, and covering it with a piece of flannel, place the patient's ear over it, or applying by binding it to the ear, as the little one is very restless and tosses about. If there is no hope of the resolution of the inflammation, then moist heat should be applied by means of a large hop poultice covered with oil silk, and renewed when it becomes cool. After using all the ordinary means to prevent and control inflammation, and none are of so much importance as those which afford relief to pain and quiet the excited nervous system, I administer to children the syrup of chloral with bromide of potassium, or in place of the chloral the camphorated tincture of opium; while to the adult I find nothing so prompt to relieve the agonizing pain as morphia sulphas and atropia sulphas, the first in doses of $\frac{1}{12}$ to $\frac{1}{8}$ of a grain, and the latter in $\frac{1}{150}$ of a grain to $\frac{1}{100}$, combined with a minute portion of the sulphate of iodine in solution, and administered hypodermically. Some good authorities, like Politzer, prefer to give the morphia in $\frac{1}{12}$ of a

grain at night and repeated; but in many instances we have found it rejected by the stomach. If we have not been successful in checking the disease, and we find that the inflammation is followed by perforation, we have then purulent otorrhœa, which, if not checked, may involve the posterior membrane, and at times the mastoid process and cells, and if there is great dizziness present the labyrinth is apt to be involved. A case of the first kind, abscess, in the posterior fold or Shrapnell's membrane, of the drum-membrane, I opened an abscess this day from a neglected catarrhal inflammation with prompt relief to the tumulus. Again we have a sub-acute inflammation of the middle ear, but no perforation or severe inflammation of the posterior surface of the drum-membrane, an effusion of blood or serum takes place, which, owing to the transparency of the membrane, can be seen as a yellow reflex with a dark hair-like line. This dropsy is to be treated by the use of the inflator of Politzer, the nozzle being carefully introduced into the nose, a little water in the mouth, and with head carried forward and to one side, so as to facilitate the passage of the fluid contents into the pharynx; the patient swallows, and the air is propelled into the middle ear, and if successful the bubbles of fluid can be seen behind the surface of the drum-membrane.

Some physicians resort to paracentesis, even when the Eustachian tube can be opened by the air-bag. Now, I am one of those who consider this operation a very simple one, and any physician can perform it if there is a good, large, not swollen meatus, and a speculum good light and forehead mirror; yet my experience is against its frequent performance in inflammation of the middle ear, as it often destroys the membrane entirely, or leaves a persistent and troublesome otorrhœa or chronic suppuration of the middle ear in scrofulous, tuberculous, or nervous patients.

I have also noticed bad results follow the use of local depletion in this same class of patients by leeches or cups.

But I approve of and perform paracentesis, and recommend it in healthy children and adults, when from the appearance of the membrana tympani there are sure indications the perforation is about to occur, when there is a yellowish green discoloration of the membrane with bulging forwards, with a livid red swelling and intense pain radiating all over the side of the head and face.

DR. G. G. DAVIS: I am surprised that in this discussion the use of hot water has not been advocated more strongly. A quart of water allowed to run into the ear from a nasal douche will usually be sufficient. It is a plan recommended by Roosa in his treatise on diseases of the ear, and is one that I have tried with satisfaction. It is usually all that will be required for most of the milder cases and many of the severer ones. I think it should, usually, be tried before resorting to the severer procedures of leeching and puncture of the membrane.

DR. HEYL: The reference which was made by Dr. Burnett, in the opening remarks, to a class of middle-ear affections which, though in many features suggesting acute catarrh of the tympanic cavity, are really pneu-

matic disturbances, is illustrated by a case which I have now under treatment. Some two weeks since I was sent for by a gentleman in the upper part of the city, with the request to bring instruments for cleansing his ear, as he was sensible of great pressure upon the drum-head which, perhaps, might be due to wax. He gave me the following history: Several days previous to my visit, he had noticed on retiring to bed a sticking sensation in the ear; during the night this increased to a violent pain. In the morning he began to hawk and spit mucus or mucopus mixed with blood. After two or three days, on attempting to rise from bed and walk, he was obliged to vomit freely; this was apparently due to the effort of walking or maintaining the upright position. About this time I saw him. I found the pain much lessened, but a great sensation of pressure in the ear; hearing dull, noticeably so to the patient, who says that it has always been acute. Watch, $\frac{1}{36}$. Examination of the drum showed some injection along the malleus handle and membrana flaccida. Membrana tympani lustreless. Relief experienced from cautious inflation with the Politzer method. Under appropriate treatment the patient is progressing toward recovery. Now, what was the diagnosis of this case? At first I was inclined to think it was a case of catarrhal tympanitis, and rather expected to see perforation take place with the formation of pus. But on observing the progress of the case, I came to the conclusion that the real difficulty was about the pharyngeal orifice of the tube, which so affected the muscular arrangements of the ossicles as to interfere with the normal pneumatic condition of the middle ear, which I believe depends in a measure on the proper action of these muscles—at least those connected with the palate and Eustachian tube. I may simply refer to two other symptoms which this case presented.

1. A difficulty of maintaining equilibrium; a sensation as if walking on a rocking boat, and the disposition to vomit which accompanied it. This, doubtless, was referable to the semicircular canals, but a curious feature was that it was very much aggravated by the vibrations of the tuning fork placed on the mastoid. This effect of the tuning fork probably often occurs in middle-ear troubles, although I do not remember to have it so marked as in this case. It suggests the thought of the susceptibility of the nervous connections of the semicircular canals to sound-waves.

2. There were slight psychical symptoms in this case, such as great indisposition for work, or conversing with any one; a sense of uncertainty about the ability to perform work; a sense of mental instability. Careful observation will probably show that a very close connection exists between abnormal mental symptoms and abnormal intra-aural conditions.

DR. RISLEY: I feel indebted to the lecturer for calling forth a discussion on this very practical theme. Any scheme looking toward the hardening of oneself against cold-taking, should be very carefully scrutinized before its adoption. Regarding the operation for paracentesis of the drum-head, I think its importance has not been set forth with sufficient emphasis in the paper of the evening. I agree fully with Dr. Burnett in the views he has expressed regarding the rare necessity for its repetition.

In the treatment of acute catarrh of the middle ear, strict regard should be paid to the stage of the disease. At the beginning of the acute inflammation, the general treatment is of great value, and he regarded tinct. of aconite, syr. of ipecac, spts. ætheris nit., etc., quite as important here as though the inflammation had attacked the mucous membranes elsewhere, *e. g.*, the bronchi. The aconite is an especially valuable adjunct to the local treatment employed.

To guard the patient from all exposure is quite as important for the successful treatment of a severe acute catarrh, involving the Eustachian tube and tympanum, as in the treatment of a bronchitis or pneumonia; confinement to bed between woollen blankets should in bad cases be rigidly insisted upon. The local treatment is of great value, but must be directed with due regard for the stage of inflammation and its severity.

In mild cases, freedom from exposure and an aconite mixture, with a little ipecac, will be quite sufficient, if the nasal passages and pharynx are kept free. If the attack is more severe, showing marked swelling of the mucous membrane of the pharynx and nose, and is attended with pain, the efforts for relief from the intense suffering are first in demand.

One of the simplest methods for relieving the pain—one always at hand, and in his experience one of the most effective—is the application of hot water applied to the external meatus. It must, however, be *hot*. It need not be applied forcibly or in large quantities. To pour the water in from an ordinary *pipette*, the head being held well over to the opposite side, is usually quite sufficient. The temperature of the water must be kept up by frequent repetition of the process, until the pain is relieved, which very soon results if the plan is to be successful. It, however, in some instances, seemed to aggravate the suffering of the patient. If relief does not follow the thorough application of the hot water, the leeches are applied in front of the tragus, as recommended by Dr. Burnett. If the inflammatory process is not arrested, there soon follows, especially in badly nourished or feeble persons, a stopping of the middle ear with mucus and exudates, with great aggravation of the patient's suffering. It is at this stage of the disease that paracentesis is usually beneficial, and very frequently urgently demanded.

The appearances of the drum-head under these circumstances, the results of paracentesis and the usual features of this disease, I will illustrate in the case of a little girl 9 years old—at the present time under treatment, but convalescing from a violent attack of acute catarrh of the middle ear, Eustachian tube, pharynx and nasal passages. She was at the time under treatment for a subacute attack, which had caused tinnitus and some hardness of hearing. She had the day and evening before the attack gone through the excitement, fatigue, and possibly exposure, incident to a birthday party. In the night severe pain came on in the right ear, and the nose was stopped by an acute coryza. Sweet oil and laudanum were poured into the meatus, but without relief. The following day was stormy, so that she was not brought for advice until the third day, and was then found still suffering so greatly that she was crying with pain. The meatus was very tender, so

that traction upon the auricle aggravated the pain. The membrane of the tympanum was a brownish red or copper color, and the superior posterior quadrant was bulging outward and downward; at the centre of this almost bagging part of the membrane was a yellowish red point. I did not hesitate to perforate the drum-head at this point, and a moment later with the air-bag was able to force through the incision a few drops of a nearly chocolate-colored serum. The pain was relieved immediately. The meatus was greatly cleansed, a wick of borated cotton, saturated with a solution of atropia sulph., placed in contact with the drum-head, and allowed to remain, the pharynx and nasal passages washed with a weak solution of sulphate of zinc, and the child sent home, with instruction that she be placed in bed and the aconite mixture given if she should be feverish in the evening.

The following day she was seen at her home; there had been no return of the pain; in forty-eight hours the wound in the tympanum was closed, and at the end of a week she was quite recovered, even of the subacute catarrh, for which she had been under treatment before the violent onset of the acute inflammation.

A thorough and careful attention to the coryza, which very usually precedes these inflammations of the middle ear, would prevent their occurrence in most cases.

DR. F. J. BUCK: I have often relieved severe ear-ache almost instantly by placing some loose cotton in the bowl of a pipe; then moisten the cotton with sulphuric ether and blow the vapor into the ear through the pipe-stem.

DR. WM. T. TAYLOR: To relieve the ear-ache of children, I have used chloroform in a similar manner to that described by Dr. Buck.

DR. WM. S. LITTLE: It would be difficult to lay down any rule that would apply to all cases in the process of hardening the system so as to prevent catching cold. The sensibility of the skin varies so in individual cases, the effect of heat and cold, the action of irritants upon the skin producing different results in individual cases; the Turkish and Russian bath not being allowable in many cases.

The hardening of the system by exposure has been advocated by non-medical men, who have been close students of nature, but the *genus homo* does not thrive as other *genera* do in undergoing the process.

The treatment of ear affection following catching cold is often rendered more easy, in addition to the methods already discussed by the author of the paper, and gentlemen following in the discussion, by inhaling medicated vapors; compound tincture of benzoin in boiling water being a very agreeable and soothing medication.

The inhalation of burnt brown sugar is a very homely but a very valuable method, several cases of severe catarrhal conditions of the throat and ear being cured by persons who had resorted to various plans of medication prior to their working in the refining room of a sugar factory.

RECURRENT IRITIS, AND ITS RELATIONS TO CHOROIDAL DISEASE.

Read March 12, 1884.

BY S. D. RISLEY, A. M., M. D.

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THE general features of iritis are so well understood that I shall not delay you in order to portray its usual manifestations; but enter at once upon the study of a small group of cases which shall serve me to illustrate the subsequent history of many eyes, once the subject of acute iritis.

The cases are selected, not because they present unusual symptoms or a novel history, but rather because they furnish clear-cut examples of a frequently occurring form of disease; and I believe throw some light on the pathogeny of chronic iritis. They furthermore illustrate the serious importance of the disease and the value of certain methods of treatment.

No form of eye disease numbers a larger percentage of blind people among its victims than those insidious, but none the less destructive processes which follow in the train of the acute plastic inflammations of the iris. It has long been observed that eyes, once the subject of this disease, are liable to progressive deterioration of vision, and often suffer from acute relapses of the iritic inflammation. This liability to recurrence seemed to depend upon the presence of attachments between the pupillary margin of the iris and the anterior capsule of the lens, since it was observed that cases which recovered, having escaped the formation of posterior synechia, but rarely showed such tendency.

Von Graefe, in 1856 (*Archiv. f. Ophth. Bd., ii, Abt. 2, S. 202*), published his important communication on iridectomy as a means of treating chronic iritis and irido-choroiditis, in which he asserts "*that the principal cause of the recurrences of iritis is the existence of synechia, especially when broad and inextensible.*" Again: "*that the exclusion of the pupil is the point from which the further complications proceed, especially chronic choroiditis.*"

The method of treating acute iritis by mydriatics, at that time insisted upon by him, had for its principal motive the prevention of adhesions of the iris to the lens capsule, and up to the present time is the universally accepted treatment.

The baneful influence which he ascribed to the existence of synechia seemed so thoroughly in harmony with the clinical features of chronic iritis, that for many years its entire correctness was accepted without a question.

Schweigger, however (Handbook of Ophthalmology, American Edition, pp. 335, Phila., 1878), in commenting on relapsing iritis, employed the following language: "Such patients are for an indeterminate length of time, at intervals of a month or longer, attacked more or less severely with iritis. It is not strange that such persons generally have a number of iritic adhesions; and still this fact is the only ground upon which is based the generally accepted assertion that these adhesions are the cause of the relapses." In substantiation of this statement he cites a fact within the range of experience, of all ophthalmologists certainly, that many persons, notwithstanding the presence of numerous synechia, do not suffer from relapsing iritis, and that, on the other hand, other cases do thus suffer, who have been properly treated from the beginning with atropine, in which no synechia remains.

F. Schnabel (Knapp's Archiv., Oph. and Otol., vol. v) has also called in question the truth of Graefe's first proposition, in an elaborate paper on the "Accompanying and Consecutive Diseases of Iritis."

Experience, however, has established upon so firm a foundation the value of the early employment of mydriatics, that we should accept only after the closest scrutiny, any report which would change our notions regarding the hurtfulness of iritic adhesions. It is, nevertheless, important that our eyes should not be closed to other conditions which may stand in a causative relation to recurrent iritis. Any doctrine accepted without question antagonizes further progress.

One object in presenting this paper is to set forth the probability *that too much importance has in some cases at least been given to the existing posterior synechia; since the plastic iritis resulting in their formation is itself often consecutive, and the subsequent eye history was probably but a continuance of the primary disease, the recurring attacks of acute iritic inflammation being only the extension forward of acute exacerbations primarily affecting the structures posterior to the iris, most probably the ciliary body and choroid.*

The following case will illustrate the truth of this proposition :

CASE I.—Mrs. H., æt. 63, in good health, although from gouty ancestry, consulted me in April, 1879, for failing sight and inability to use her eyes for near work without pain : $V = \frac{20}{XL}$ in each eye. A weak concave cylinder was very positively selected with its axis at 180° , but no smaller letters could be seen. She complained of a fine web or veil before her eyes, dotted with numerous fine dark points ; externally the eyes appeared normal except some enlargement of the anterior perforating vessel ; the field was perfect and no increase of tension. The ophthalmoscope revealed only a hazy view of the eye-ground. The nerve was normal in color, and there was no derangement of the central retinal circulation ; the visible choroid showed no change. The cornea and lens were transparent, but the anterior part of the vitreous body was hazy, and a magnifying glass showed innumerable fine dark points as viewed by transmitted light. No web could be made out and the dust-like particles were almost fixed. The condition of both eyes was similar.

There was no marked change after four months, during which time she had been quite unable to use her eyes. The following August, after a drive in the park, facing the western sun on a hot afternoon, she returned home with much discomfort in O. D., which during the night ripened into severe pain, which prevented sleep. The following morning, September 1, the eye was red, painful and extremely sensitive to light. September 2, the eye was some better and she ventured out of doors, the day being cloudy, and came to the office. There was still marked ciliary injection, ball tender to the touch in the ciliary region— $T n - V$ diminished, and mydriasis revealed a broad synechia, attaching the lower pupillary margin to the capsule. She proved remarkably susceptible to all the mydriatics, so that I was compelled to use them with great caution to prevent serious constitutional poisoning. The eye rapidly regained its former condition, but the adhesion remained throughout the subsequent history of the case. Through the following autumn and winter Mrs. H. had repeated attacks of inflammation resulting in the formation of delicate synechia, but profiting by her first experience, the mydriatic was applied at the very outset, so that they were usually torn asunder, leaving a minute pigment spot on the anterior capsule at the site of the attachment. It is worthy of remark that however early in the attack the mydriatic was used, this attachment was, with a few exceptions, found present. The attacks occurred quite indifferently in either eye, often in both simultaneously, rarely more than ten to fifteen days elapsing without an out and out attack of inflammation, or a flushing of the ciliary region. These exacerbations were determined by various causes ; *e. g.*, fatigue, a shopping expedition, any effort at near work or a visit to the laundry or kitchen, immediately brought on pain and ciliary injection, resulting in an iritic attack if persisted in.

Under the internal administration of guaiacum and iodide of potassium, and the free use of the Bedford water, the attacks gradually grew less fre-

quent and severe. 1880 was passed with only a few exacerbations, but she purchased the immunity from attack by the most assiduous care, in the avoidance of those influences which experience had taught her to dread. The balls, however, remained tender and her vision fluctuated about $\frac{20}{XL}$ sometimes much less than this, at other times somewhat better; on one occasion with correcting glass it was noted as $V = \frac{20}{XXV}$ in each.

In October, 1881, while sitting in church, she had marked photopsies, which were followed in a few hours by an usually severe onset of binocular iritis, which resulted in firm bands of adhesion in both eyes, notwithstanding the employment of local depletion and of atropia as vigorously as she could bear it. The eye once more lapsed into its former condition, with an additional adhesion in O. D., and a firm band in O. S. There was after this attack a marked increase in the haziness of the vitreous humor. Her former experience was now repeated. There were frequent recurrences of the iritic trouble, so that she lived in constant dread, and was greatly discouraged. Various therapeutic measures were adopted, only to be relinquished as of no avail. Iridectomy was now suggested as a probable means of permanently removing the tendency to recurrence. Sulphate of eserine had been used at different times, but only with the result of adding acute periorbital neuralgia to her present distress.

At the time I chanced to be interested in a series of clinical observations at University Hospital, on the effect of this drug over the nutrition of eyes lost by past injury or former inflammation, but now subject to subacute exacerbations and setting up sympathetic irritability of the fellow-eye. I had noticed that while solutions not stronger than 1 gr. or 2 gr. to the f^3i , would cause great pain and thus prevent its use, weaker solutions could be used without difficulty, and with the effect of rapidly relieving the redness of the chronically inflamed eye, and with it the irritability of its fellow.

A very striking result had been gained in a case then and still under observation. A young woman, at work in a carpet mill, came to the clinic, having been advised to have her left eye removed, to which she strenuously objected. There was well-marked sympathetic irritation, depending upon the irritable and blind left eye. The cornea of O. S. was opaque, still vascular and becoming staphylomatous at the centre, where was an adherent circatrix at the site of a perforating ulcer occurring in childhood. The eye was painful T +, and the sunlight was very painful to the right eye. She could not come into the hospital for immediate enucleation, and was very loth to submit to the operation at all, so she received a solution of sulphate of eserine, gr. $\frac{1}{4}$ to f^3i , to be used in the blind eye three times in the twenty-four hours, and instructions to report daily. All the symptoms rapidly subsided. The drops were used for many weeks, and although nearly two years have elapsed, she has had no serious return of the symptoms, notwithstanding the presence, on one occasion, of a small foreign body in the

centre of the former corneal staphyloma, which had been allowed to remain many days.

Without room for question, the nutrition of this eye had been greatly improved under the use of the eserine. With this and other examples in mind, Mrs. H. was directed an eserine solution, .002 gm., 10 c. c., to be used twice daily. In a short time after each instillation, she had a throbbing in her eyeballs and a sensation which she described as a starting forward of the eyeball, which then appeared to recede to their proper bed. There

was also a slight twitching of the lids, but no pain. A month later $V = \frac{20}{XX}$

with her correcting glasses in each eye. She had not been even threatened by her old enemy, and was already gaining confidence in the use of her eyes at near work. Four months later, after a long and fatiguing railroad journey, she had a mild attack in Cincinnati, but she had not neglected her homatropin solution, which, through her long trial, she had used with the earliest symptoms of an attack. This was freely instilled, and in twenty-four hours the eyes were once more quiet, and she returned to the eserine. After a lapse of two years there has been no recurrence, notwithstanding the existence of firm inextensible synechia in both eyes. The vitreous cleared up so that no trace of the former trouble could be detected. The choroidal margin at the optic nerve showed some increased pigment absorption, but no other change was noted. The ant. perf. ves. also assumed their normal condition.

I am convinced that had we the opportunity more frequently to study carefully iritic eyes before the onset of the acute and painful trouble, we should find the attack had been preceded by an interval of dim or weak sight, with disease of the choroid or retina, or both. In the case of Mrs. H., I had the opportunity to observe, first, the haziness of the vitreous body, and to follow subsequently the painful history of relapsing iritis extending over more than three years. These attacks were invariably preceded and accompanied by increased dimness of sight, due to increased opacity of the vitreous humor, as I had repeated occasion to witness. I did not hesitate to diagnosticate disease of the ciliary region of the choroid-tract, notwithstanding the fact I could discover no change in the choroid as far forward as I could study it with the ophthalmoscope. I was, however, borne out in this by the fact of the ciliary tenderness which persisted throughout, by the dilatation of the anterior perforating vessels, and by the material poured out at each exacerbation, into the vitreous. This opinion was strengthened furthermore by the inability to use the eyes at a near point.

The opinion that iritis is often secondary to choroiditis or cyclitis receives confirmation in the history of the following case :

CASE II.—W. F., æt. 40 years, engineer on coastwise steamer, came to eye clinic of the University Hospital in April, 1878, with commencing iritis. A month before V. in O. D. had failed, and he had a floating web before it. The iris was discolored and he was suffering from great pain and dread of light. The pupil, however, dilated large medium under atropine, and the vitreous was filled with large grayish webs and black masses, which floated freely about with the movements of the eye. V O. D. counts fingers. In O. S. $\frac{20}{\text{XL}}$. He gave a clear syphilitic history, and was just recovering from mucous patches in the mouth. He was admitted to the wards of the hospital, and after a tedious treatment finally recovered without post. syn. and V — $\frac{20}{\text{C}}$.

During his residence in the hospital he complained of failing V. in O. S. —V — $\frac{20}{\text{LXIV}}$ —veins of ret. large and tortuous, and the retina hazy throughout. Three days later V. had sunk to $\frac{20}{\text{LXXX}}$, and the vitreous was filled with numerous floating opacities.

Under mercurial inunctions these rapidly cleared up and he was discharged June 20, with O. D. V $\frac{20}{\text{LXIV}}$ O. S. $\frac{20}{\text{XXX}}$. The following December he returned with failing V—dread of light and cil. redness. He remained under observation for over two years, in the meantime having repeated attacks of iritis. He carried with him a sol. of atrop. sulph., which was applied as soon as he noticed any aggravation of his eye trouble, and thus prevented the formation of adhesions to the capsule. He had many more relapses of dim sight, on three separate occasions affecting the left eye also, which, however, never resulted in iritis. The patient was an intelligent man, and soon learned to treat himself with much skill. He had learned that, unmolested, his attacks of increased impairment of vision in a few days resulted in attacks of iritis. He therefore did not wait for the attack, but began treatment at once. He would swallow a saline cathartic, instil his atropine solution, don his smoked glasses, give up all attempt at near work, and quietly await results. If the attack threatened notwithstanding these measures, he applied leeches which he carried with him, while at sea, as a precautionary measure. The attacks in the left eye were ushered in by flashes of light, and either a hazy retina or cloudy vitreous, soon to be followed by floating opacities.

In this case there can be no question but that the recurring iritic attacks were consequent upon increase of the choroidal disease. Furthermore, the recurring attacks of iritis both in

Case I and Case II, demonstrate the truth that relapses of iritis are not necessarily dependent upon the presence of synechia, and lend support to the assertion of Schnabel (*loc. cit.*), that "experience has taught me that eyes possessed of synechia are no more liable to relapses of iritis than those which have passed through an iritis without retaining any synechia."

I have been greatly interested in the retinal condition present in irido-choroiditis. In Case II the attacks in the left eye were attended with all the necessary manifestations for a retinitis. It is improbable that any serious disease of the choroidea can exist without leading to a pathological condition of the retina, and doubtless the reverse may be true. But it must be remembered that any study of the eye-ground in iritis presents great difficulties because of the almost constant presence of semi-opaque media and the great dread of light experienced by the patient. In my own experience I have been more frequently thwarted than successful in the attempt to do so. When extensive synechiæ are formed, there is also very frequently a web-like opacity of the anterior capsule which precludes any satisfactory inspection of the tissues beyond it.

Notwithstanding these difficulties, Schnabel (*loc. cit.*) asserts that "the ophthalmoscopic examination of individuals suffering with acute iritis revealed, no matter what the cause might have been, most frequently diffuse retinitis." He appends a history of twenty-three cases of acute iritis thus examined. Five of the cases were diagnosticated as having both retinitis and hyalitis. With reference to this group of cases he states that: "The most remarkable fact demonstrated by the foregoing table is the *almost constant existence of retinitis with acute iritis.*" Although he acknowledges that in the vascular system of the retina (in iritis), we do not see any remarkable changes. Among sixteen eyes having specific iritis he found only one normal retina; and among ten having non-specific iritis only three.

However strongly we may feel inclined to call in question the perfect accuracy of a diagnosis of diffuse retinitis, made through a muddy vitreous humor, particularly when there are no "remarkable vascular changes" to verify the retinal complication, still it must be acknowledged that it is comparatively rare to find an eye affected with acute iritis, that has normal sharpness of sight, or in which the media are found transparent. The existence of

opacities in the vitreous are, I think, by most men accepted as evidence of the presence of choroiditis. Schnabel (*loc. cit.*) however, does not accept this dictum, but insists upon ophthalmoscopic verification of the choroidal disease for a diagnosis of its presence, and insists upon primary inflammation of the vitreous itself. In his study of twenty-three cases of acute iritis, therefore, he found "most frequently retinitis, comparatively seldom the presence of changes in the vitreous, and most rarely anomalies in the choroid."

The anatomical relations of the iris to the uveal tract, of which it is in fact an extension, would lead us *a priori* to anticipate a frequent connection between the diseases of the one and the other. Indeed it was hardly to be expected that an acute inflammation of the iris could be rigidly confined to that important structure, lying as it does in such close proximity to the ciliary body and muscle, enjoying the same blood-supply. But it is equally probable that diseases affecting the choroid proper or ciliary region, might also, by the simple fact of continuity of tissue and blood-supply, attack the iris secondarily or simply by a gradual process of extension forward. That such extensions of inflammation can and do occur has, I think, been demonstrated by the foregoing cases. It receives further confirmation in the following case, which will serve also to demonstrate the serious importance of this disease and the necessity for prompt and well-directed treatment.

There can be no question of the great importance of preventing the adhesion of the iris to the capsule, for however little influence the attachments themselves may have over the production of subsequent attacks of iritis, there can be no doubt as to their baneful influence when they by frequent repetition finally lead to exclusion of the pupil; this will be painfully illustrated in the history of Case III. I have already presented to the Society the early history of this case, as one of a group illustrating the history of secondary glaucoma. I now repeat it more in detail with the subsequent developments:—

CASE III.—Mrs. D., æt. 46 years, consulted me August 17, 1881, her vision being so seriously at fault that she was led into the consulting room by her husband. She gave the following history: Her first eye trouble was experienced in March, 1880, when she quite accidentally discovered that vision in O. D. was impaired. The impairment steadily progressed without pain or other symptoms until the following July, when she was attacked with violent pain in the right eye, which spread over the entire

right side of the head. The eye was red, tender to the touch, photophobia so intense and pain so severe that for eight days she was forced to remain in bed in a dark room. Her attendant was a homeopathist.

The impairment of vision increased rapidly after this attack; there were frequent subacute exacerbations which simply added to her constant discomfort, and were characterized by an increased dimness of vision, exaggerated tenderness and injection of the ball. The left eye had given no signs of trouble until April, 1881, about one year from the commencement of impaired vision in the right. She then noticed diminished acuteness of vision which steadily progressed up to the present time, but with no violent onset of inflammation. It had had, however, frequent attacks of redness, attended with periorbital pain, dread of light, soreness of the ball, etc. Mrs. D. to all appearances was in perfect health; she had married late in life and had enjoyed uninterrupted health; had never been pregnant; the menstrual function had been regularly and painlessly performed until the present month, which she had missed for the first time, and was now annoyed by alternating flashes of heat and perspiration.

In O. D.: T + 2, cornea steamy, and sensibility markedly diminished, some ciliary injection, anterior chamber shallow. The iris was atrophic and fixed; pupil 2 mm. in diameter, and occupied by a grayish white web, and pupillary margin of iris attached in annular synechia; only grayish red light from fundus; there was no pouching of the iris; counts fingers only with difficulty and as shadows, the hand being held between the eye and source of light. Field taken with candles shows perception in temporal field only.

O. S.: Cornea also steamy, sensibility somewhat diminished, some ciliary injection, anterior chamber shallow, iris discolored, nearly annular synechia, no pouching, T + ? F perfect, $V = \frac{2}{CC}$. Solution of eserine sulphate 1 gr.

f 31, relieved somewhat the periorbital neuralgia, and the steaminess of the cornea diminished, but there was no improvement in the vision.

On August 31, I did a broad iridectomy upward on the left side, and a large sclerotomy on the right, in the hope of diminishing the tension and relieving the pain, as I had no hope of restoring useful sight in the right eye. The iridectomized eye recovered slowly and with considerable reaction, while the right recovered from the sclerotomy without any reaction, and with entire relief of pain. Three weeks after the operation, O. D. white, T. n; free from pain; cornea transparent; pupil as before; ant.

chamber normal; but much to my surprise V had risen to $\frac{20}{CXXVI}$

and the field now taken on perimeter had extended in all directions. O. S. showed typical coloboma; ant. chamber normal; T.—; V, quantitative perception; cornea transparent; V steadily improved in both, so that three months after operation, during which she had steadily used the weak

eserine solution. O. D., $V = \frac{20}{LXIV}$. O. S. $\frac{2}{C}$, and she visited the office without a guide.

The improvement continued until, notwithstanding occasional attacks of redness, the following September, 1882, one year from the date of operation, when there began an insidious onset of serous iritis in the sclerotomized eye, with punctate deposits on Descemet's membrane and pouching of the iris without increase of tension, and V sunk to $\frac{15}{\text{CC.}}$

O. S. as before. On September 26th I did a broad iridectomy in O. D. upward, leaving a faultless coloboma, and liberating a quantity of yellowish glutinous fluid from the posterior chamber. The eye recovered with but little reaction, and all went well until in the night following the fourth day of the operation, when she was awakened by an acute pain in the eye. The following day the anterior chamber was filled with blood. The clot slowly absorbed, the eye softened, and she gradually lost all perception of light. In a few months deposits appeared in Descemet's membrane in O. S. and without pain, V steadily failed until only merest quantitative perception remained.

During these months she suffered much from the troubles associated with the menopause, which doubtless had to do with the unfortunate termination to a most interesting pathological history.

The case furnishes wide scope for speculation regarding its pathogeny. Her failing vision at the outset was surely not iritis, but some disease of the deeper tunics, most probably retine choroiditis. The following violent attack of iritis, resulting in adhesion of the iris to the capsule, the subsequent subacute exacerbations, with the formation of additional bands of lymph, until exclusion of the pupil was reached; then the appearance of secondary glaucoma, with the increased tension of the ball, the cupping optic nerve, the contracted field of sight and almost total blindness; the rapid relief of all the symptoms, by simply incising the angle of the anterior chamber, to be followed by widening of the field and a large restoration of sight, notwithstanding the fact that the gray film still covered the pupil and the iritic adhesions remained: furnishes a picture of disease that cannot fail to interest, although in many respects it baffles a satisfactory explanation. Had the early treatment been so conducted as to prevent the formation of the synechia, until such time as the primary inflammation of the deeper tissues should subside, there is no question but that the ultimate result would have been very different. For in that case the secondary glaucoma would doubtless have been omitted from the picture of disease.

In O. S. the synechia was not annular, so that there was still some small communication between the posterior and anterior

chambers. The result of the iridectomy in this eye was disappointing, as compared to the brilliant, though unexpected, but unfortunately temporary, result in the right eye.

The rapid improvement in O. D. can probably be explained by the relief of the glaucomatous tension afforded by the sclerotomy. Notwithstanding this, the choroidal disease slowly progressed toward a fatal issue. It is to be regretted that iridectomy had not been done instead of sclerotomy. The only excuse was that the eye was regarded as hopelessly blind, and the operation was designed to relieve the suffering of the patient.

In the light of comparatively recent investigations, the progress of the disease from the right to the left in the first instance, and the onset of serous iritis about three months after the loss of O. D., furnishes a very interesting history in the light of recent investigation into the pathology of sympathetic irritation.

If in these remarks I have succeeded in making clear that iritis is more than an inflammation of that delicate membrane, I shall feel that I have accomplished my purpose. I do not wish to assert that all cases of iritis begin in the choroid and ciliary organs, but do assert that many exist only in common with inflammation involving the deeper structures, and that their treatment, therefore, should be conducted with this fact in view.

There are many cases in which no evidence of inflammation of the retina or choroid can be gained as having preceded the attack of iritis, where, nevertheless, after the subsidence of the iritis the deeper trouble was revealed. That both may have been due to a common cause, or that the trouble simply extended backward by virtue of anatomical relations, cannot be denied. But that even in simple idiopathic iritis, attacking the iris primarily, there is also profound congestion of the entire choroidal tract is unquestionably true, and it would be folly to deny that this congestion might under favoring conditions, *e. g.*, existing dyscrasia, pass over into a pathological state, involving both choroid and retina.

1630 WALNUT STREET.

DISCUSSION ON RECURRENT IRITIS.

DR. SHAKESPEARE, in opening the discussion, said: I agree with Dr. Risley as to the importance of a knowledge of the pathology of these troubles, but I am not entirely in accord with him when he suggests that recurrent iritis is most frequently the direct consequent of extension of inflammation from the choroid.

My own observation, and even some of the cases related by Dr. Risley, create in my mind the very strong impression that recurrent iritis, when not directly caused by irritation due to suddenly and constantly checked movements of the iris, where a partial synechia exists, is frequently brought about by constitutional conditions which affect the iris quite as directly as they do the choroid. I refer, for instance, to the agency of the syphilitic or rheumatismal poison in the production of inflammations, and express my belief that when recurrent iritis is not the direct result of the combined irritation of posterior synechia and of a disturbance so slight or transient as in itself to be usually incapable of exciting deep inflammation, it is very often induced by the action upon the iris of a specific irritant, such as the virus, whatever that may be, of syphilis or rheumatism. I can see no valid reason for assuming that recurrent iritis and choroiditis, when associated or single, and occurring in a rheumatic, gouty or syphilitic patient, are not each caused by the same constitutional irritant.

I by no means deny that there are cases of recurrent iritis, where the inflammation of the iris appears to be an extension of an acute, or of an exacerbation of a chronic inflammation of the deeper portions of the ureal tract.

But Dr. Risley seems to be of the opinion that such is the customary origin of recurrent iritis. It is on this point, then, that I differ from him. Moreover, the author, reasoning from his view of the usual course of recurrent iritis, arrives at the conclusion that adhesions of the pupillary margin of the iris and the adjoining capsule of the lens, so long as the whole pupillary border of the iris is not bound down and the communication of the anterior and posterior chambers of the eye thus closed, are comparatively insignificant matters and should ordinarily occasion little or no apprehension. Here again I must differ from him, and still more positively than before.

I regard it as all-important in the treatment of primary iritis to guard most carefully against the formation of posterior synechia. Furthermore, I consider it important, after the second attack has established the recurrent nature of the malady, and at a favorable moment, to carefully separate the synechiæ if they are not too extensive, or, in the latter case, to remove a portion of the iris by iridectomy.

Recurrent iritis after synechia is sure in time to bring about complete occlusion of the pupil, a condition which, unless remedied by operation, is certain to prove most disastrous. In order to remove the constant irritation of the iris, caused by one or more bonds of adhesion to the lens, and, still more important, to remove as far as possible the risk of subsequent complete occlusion of the pupil, I do not hesitate to advise the separation, at a proper moment, of slight adhesions, or the performance of an iridectomy if they are very extensive.

I am very well aware that occasionally (rarely indeed) the capsule of the crystalline lens, at the point of attachment, may be torn in the effort to detach the synechiæ and a traumatic cataract be established. But the danger is, I think, so small in comparison to the danger that the eye may be ultimately destroyed if the synechiæ be left to themselves, that it is, as a

rule, safer to follow the practice of removing synechiæ when recurrent iritis is once confirmed. Of course the presence of a constitutional irritant greatly complicates this problem. In the cases, which I believe are comparatively rare, of recurrent iritis by extension of inflammation from the choroid or ciliary body, it is possible that the best procedure would be to leave posterior synechiæ alone so long as they are not complete, and do not occlude the pupil.

DR. LITTLE : Dr. Risley's paper is very important and suggestive, for the early recognition of iritis and its proper treatment prevent or mitigate the serious results that follow in eyes thus affected.

The vascular tissues of the eye, composed of choroid, ciliary body and iris, are to be considered as one, and to say that inflammation is limited to only one part of it is difficult.

The treatment is largely mechanical, that is the whole tissue when inflamed should be made free from muscular action ; the iris dilated so no adhesions occur ; the ciliary muscle and choroid likewise passive, so as not to damage the retina or vitreous ; after subsidence of the inflammatory processes, if optical defects exist they should be corrected, so as to prevent recurrent attacks from irritation and congestion that exist in the tissue when optical defects exist, for the effect at seeing produces trouble in these cases, and gives a foothold in a congested tissue for the constitutional taint, specific, rheumatic or otherwise ; mere exposure will produce the same result. The constitutional treatment is more effective in eyes free from strain and normally vascularized.

As to the use of eserine in iritis, while a few years ago it was considered courageous to use it, in certain cases it acts well, care being taken to guard against adhesion by using atropia at stated times to prevent its adhesion ; in cases where the adhesion already exists, it works well in relieving pain, but in the average case of iritis, atropia must be looked upon as the only treatment locally.

Dr. Risley's suggestion and good effect he had with eserine in a case of sympathetic iritis, while suggestive and contributory to advance in therapeutics of this disease, cannot yet be looked upon as a preventive of sympathetic inflammation of the eye, no more than eserine can be claimed as a permanent method of cure for glaucoma by its use.

The presence of hyalitic in these severe cases of iritis, presenting a view of the fundus of the eye, can be helped or cleared up to some extent by the use of electricity, so that changes in the retina or choroid can be recognized.

Opinions as to operative procedure differ in these severe types of iritis ; it is better to wait till severe symptoms subside or disappear, and yet some cases do well by immediate operation ; not enough such cases have been reported in comparison with those that have had no operation, for any judgment or statistics to be grounded ; personal experience gives different opinions to observers.

DR. L. WEBSTER FOX : I rise to give the clinical history of a case of

recurrent iritis, ending in sympathetic ophthalmia. Eighteen months ago I was asked, in consultation, to see a patient who had had several attacks of iritis in the right eye.

Mrs. E., age 33, married, well developed, with an irrelevant family history, always enjoyed good health and normal vision. The family physician three months previously was called to see the patient, who was suffering with pain in the eye-ball, accompanied by supra- and infra-orbital pain, a marked ciliary zone of congested blood-vessels, a discolored iris which was sluggish, and vision blurred. Atropia solution, grs. iv to $\frac{3}{4}$, one drop every two hours, and internal medication of calomel was given. The disease responded rapidly to the treatment; the eye regained its normal color with full acuity of vision; three weeks subsequently a second attack came on, which was again promptly treated with good results. Nine weeks after the second attack, a third was ushered in with very severe pain over the brow, down the track of the infra-orbital nerve, congestion of the sclerotic vessels, with a deeper pink zone of ciliary blood-vessels, musty brown iris and hazy vitreous, with vision reduced to $\frac{20}{200}$, other than the hazy vitreous no lesion in the fundus could be seen. The following treatment was instituted:—Atropia sulph., grs. iv to $\frac{3}{4}$, one drop in eye every three hours, internally; hydrarg. chlor. mit., grs. ii, guarded by pulv. opil, gr. $\frac{1}{4}$ twice daily, and four leeches to temple. The patient improved, and untoward symptoms passed off. The left eye was examined with the ophthalmoscope, and found normal, excepting slight degree of hypermetropic astigmatism. The eye (right) remained quiet for about two months, when another attack came on more virulent and aggravated in its symptoms. Upon instituting the atropia solution, it was found that posterior synechiæ had taken place, the pupil only responding to the mydriatic in the upper and outer quadrant, possibly to one-sixth its diameter. The media, which had regained its transparency, was hazy (vitreous), obscuring the details of fundus. The vision had fallen to counting fingers at eight feet. In addition to the calomel and opium, a mixture of hydrarg. bichlor., gr. $\frac{1}{48}$, pot. iod., grs. xx in water three times daily, was given; this treatment was continued till permanent salivation had taken place, which was in six to eight days; the hydrarg. was discontinued, but pot. iod. given as usual.

In two weeks the inflammatory condition commenced to pass off, but vision was reduced to qualitative perception only, after the inflammatory conditions of the eye had disappeared (four weeks after the beginning of the last attack); the patient was suddenly taken with great pains in back of head and at times nausea; these pains became so intolerable that the patient had to be kept under the influence of a hypnotic for several days at a time, this condition lasting off and on for four weeks, at the end of which time paresis of rectus externus of right eye manifested itself; there was convergent strabismus with diplopia, but no hemiopia could be elicited; the patient at this time answered questions intelligently; a careful examination of the right eye revealed no change either in the irregular shape of the pupil or in the haziness of the vitreous. In the left eye, however a marked change had taken place; owing to the condition of the patient, it was impossible to

make an ophthalmoscopic examination of this eye for four weeks. Well marked keratitis possetates on Descemet's membrane, iris musty brown, vitreous slightly hazy (?), swelling of the optic nerve, arteries lessened in calibre, veins full but not tortuous, their reflex gone, several small spots of choroiditis scattered about the equator of the eye, the eye presenting a perfect picture of inflammation of the ureal tract. The urine was examined, no sugar nor albumen found. Medication was pushed till pronounced salivation and iodism made their appearance; notwithstanding this treatment the eye (sympathizing) continued to grow worse until qualitative perception of light only remained (the paresis of the rectus muscle impressed). The vitreous became so filled with inflammatory products that it was impossible to make observations of the change that was going on in and about the optic nerve and choroid.

The last attack of iritis in the left eye was four months ago, when the eye suddenly became painful and exceedingly sensitive to light, this attack lasting three days, the patient having been under constant medication (pot. iod.), leeches to the temple seemed to relieve the extraordinary condition at once. The patient is still under observation, and at last examination the vision in right eye (primarily affected) was found to be $\frac{A}{200}$, at one time qualitative perception only; in left eye (sympathizing eye) qualitative perception only. I may state that this is the first case of sympathetic ophthalmia due to recurrent iritis, that I have ever seen, although I had the opportunity of seeing many cases of recurrent iritis while clinical assistant and house surgeon at Moorfields Eye Hospital, London.

Operative interference was the method adopted at the above-named institution, to protect the patient from recurrent attacks. This was done by excising part of the iris. Mr. Streatfield, at rare intervals performing this operation of separation of the synechia from the anterior capsule of the lens. The iridectomy was performed at such time when it was supposed that the iris was free from inflammation. An operation may be done with safety in from four to five months after the last attack of iritis.

DR. RISLEY, closing the discussion, said: I quite agree with Dr. Shakespeare in his estimate of the importance of systemic conditions in this disease. Not only is syphilitic disease, the rheumatic or gouty diatheses frequent causes of idiopathic iritis, but doubtless exert a marked influence in sustaining the pathological conditions involving the entire choroidal tract, as a sequel of the acute iritis. But for these diatheses, many acute cases which are followed by chronic iritis, would have returned to a state of health. It was not my desire to underestimate the importance of the iritic attachment, but to point out that as a factor in the production of recurrent iritis, it had been overestimated.

If the views I have expressed are true, it renders less justifiable the operations for their detachment, *e. g.*, that devised by Streatfield, gently tearing them away by means of a blunt hook, which he inserted between the iris and lens capsule, or the Passavant operation, which consisted in grasping the iris at the point of attachment with forceps, and by traction

detaching the adhesion. If the attachments are not so deleterious as was supposed, the risk following these operations is not to be justified.

I have no doubt but that an eye constantly in a state of retino-choroidal irritation, in low grades of inflammation, as the result of strain in overcoming a hypermetropia or astigmatism, is more ready to take on all forms of disease, and may, therefore, as Dr. Little very fitly suggested, be more liable to iritis. Certainly, once attacked by iritis, such an eye would be more prone to disease of the choroidal tract and chronic or recurring iritis.

It was not my wish to present eserine as a panacea, or even as a usual remedy to be employed in the treatment of sympathetic disease, but reported the case in my paper as a clinical fact, the design being to set forth the value of this drug in improving the nutrition of chronically influenced eye-balls prone to set up sympathetic irritation, especially where there is increased tension of the offending ball.

I have had no experience in the use of electricity as an agent for hastening the absorption of vitreous facilities. Ophthalmologists, I am sure, would hail with pleasure any safe method which would accomplish a result so desirable. I should, however, use with great caution an agent, the powers of which are so imperfectly understood in any eye with an inflamed retina and choroid.

Dr. Fox has presented a very interesting history in his case of serous iritis, followed by sympathetic irritation. Such cases seem to shed light upon the vexed problem of the pathology of sympathetic irritation. One of the most interesting and important contributions, to our knowledge, of the subject, is that by Max. Knies (Vid. Archiv. Opthal., vol. ix, page 125, N. Y.). In the case, the pathological histology of which he has so carefully presented, the disease had been apparently transmitted by the way of the optic nerves. The reported cases of sympathetic neuroretinitis are getting more numerous, and it would seem that ere long we shall have to relinquish the term *sympathetic*—certainly for very many cases of disease communicated to the fellow eye—for some name which shall indicate its true pathology.

Regarding the time after the iritic disease, when it is proper to perform iridectomy, I would remark that the symptoms in the individual case are probably the true guide for operative interference.

THREE CASES ILLUSTRATING SOME POINTS IN THE PATHOLOGY OF CERTAIN INJURIES OF THE SHOULDER-JOINT.

Read March 19, 1894.

BY C. B. NANCREDE, M. D.,

Surgeon to the Episcopal Hospital, and to St. Christopher's Hospital for Children.

A FEW preliminary anatomical points must be passed in review for the ready comprehension of my later remarks. The shoulder-joint differs in many important points from any other articulation of the body. A moment's reflection upon the almost unlimited range of movement which it enjoys, will at once suggest that the ligaments of this articulation cannot be the means by which the joint-surfaces are held in apposition; otherwise anything like freedom of movement would be impossible in a ball and socket joint where the socket is so shallow as in this articulation. What then does hold, firmly apposed, the articular surfaces? It must be something always tightly stretched, yet always capable of lengthening or rather always practically loose: Nothing but muscle could fulfil any such purpose. In truth, the muscles surrounding the joint are the most important ligaments the articulation possesses. When these are paralyzed, or in the cadaver, the head of the humerus readily falls away from the glenoid fossa. Bearing this fact in mind, you will clearly apprehend that the joint surfaces are kept pressed together solely by muscular tension. Again, the glenoid fossa, unlike the socket of any other important joint, *has no epiphysis*, which explains to a degree the fact that even in the young the head of the humerus may be so affected as to demand excision, while the glenoid process is either entirely or nearly healthy. Closely related with the scapulo-humeral joint, we find a number of bursæ, some of which commonly communicate with the joint, while others do not. To the former alone I shall devote my remarks. There is a large one between the acromial process and the coraco-acromial ligament upon the one hand, and the shoulder capsule upon the other. Two bursæ—the exact sites are unimportant for our present purposes—are situated between the subscapularis muscle and the capsule. An occasional one is placed between the infraspinatus

muscle and the capsule, into which it often opens, as do the others just mentioned. Let an inflammation be set up in these sacs, and it certainly spreads to the joint itself, should communications exist; or nearly as surely by mere contiguity of tissue, if no opening between joint and bursæ is present. The articulation is securely covered in by the voluminous deltoid, so that any direct injury to the fibrous or synovial tissues of the joint is almost impossible from direct force as a blow, although a twist *may* injure it, notwithstanding the latter is more apt to tear the bursal walls. The upper epiphyses of the humerus—of which there are three—coalesce at five years, but they leave a layer of epiphyseal cartilage between head and tuberosities and shaft, which in places either coincides with the capsular attachment or is actually within it. From these anatomical facts it must be clear that direct force, as a blow, can rarely injure the joint itself, but must either set up trouble in the surrounding bursæ, or in the epiphyseal cartilage, or in both. Once again, the interior of the joint, the muscles moving the joint, and the skin over their attachment, are all supplied by the same nerve or nerves; so that let a joint injury start where it may, the articulating surfaces of the shoulder-joint are subjected to such an injurious degree of pressure from direct or reflex muscular contraction as is possible for no other articulation. The bearing of these anatomical facts upon prognosis and treatment need hardly be pointed out.

The first specimen I proposed showing was presented to the Society some years back, and my reason for again showing it is the sharply contrasted result presented by the second specimen which I shall exhibit.

On November 27, 1878, the Society had an opportunity of examining the first patient and the admirable results of the case. To those who were not present, or who have forgotten the details, I will briefly recapitulate the history of the case, with the treatment pursued.

Compound Fracture at the Anatomical Neck of the Humerus.

The patient, J. M., æt. 14 years, fell, upon the afternoon of Sept. 23, 1878, from a tree about twenty-five feet, landing on the ground, but striking in his fall against the branches of the tree, and sustained the following injuries: The shaft of the humerus was separated from the head and greater and lesser tuberosities. The line of fracture closely followed the epiphyseal cartilage, although in several places the diaphysis was fractured. The shaft,

slightly split, was driven through the integuments over the lower part of the deltoid muscle on its anterior aspect, tearing in its course the following parts; the insertion of the deltoid was completely stripped off with the subjacent periosteum; the coraco-brachial, teres major and latissimus dorsi were in like manner torn off, the latter carrying with them the posterior lip of the bicipital groove. The tendon of the pectoralis major was torn off about half an inch from its insertion, and one, if not both, heads of the biceps were ruptured. In consequence the head and neck of the bone, deprived of periosteum, merely hung suspended by the capsular ligament and the rotator muscles. The shell of bone connected with the head and tuberosities was fissured at various portions of its circumference, as if by the impacting action of the wedge-shaped extremity of the shaft. I enlarged the wound and removed the fragments you see, viz.: $4\frac{1}{4}$ inches of the humerus, including its head. Seven weeks after the operation he could remove his coat, vest and shirt without assistance. Ten weeks after the injury considerable reproduction of bone, even up to the margin of the glenoid cavity, was observed, with new attachments for the pectoral and latissimus dorsi muscles, as determined by Dr. C. T. Hunter. The actual shortening consequently amounted to only $1\frac{1}{4}$ inches. He had perfect use of the forearm, could put his hand to his mouth, behind his back, and to his ear. Of course he had lost all over-hand movements.

The course of treatment pursued, and my reasons for deciding upon it, seem worthy of detail, since such injuries are but seldom seen, and, as far as I can discover, no clear rules have been laid down for their treatment. To the members of this Society who devote themselves especially to surgery, I need hardly say that no question of amputation arose in my mind; but to those in pure medical practice I would say that when the main vessels and nerves of a limb remain intact, the injury to the soft parts having been produced by the bone itself, not the fracturing force, almost any degree of shattering of the bones may be recovered from, in the young, without amputation. Two lines of treatment then offered for consideration, viz.: the simple return of the bone, closure of the skin-wound, drainage, and trusting the case to nature; or the resection of the injured bone. Theoretically the first would have seemed the better course, promising no shortening of the limb, and the retention, in a measure, of the power of the deltoid. In reality, however, the chances of union were not one in a thousand; and if not union, then necrosis with its consequent shortening; necrosis, too, meaning months or years of inflammation and supuration, matting the muscles together so that when recovery occurred—almost necessarily by an operation—the usefulness of

the limb would be but slight. Resection, on the other hand, offered the complete removal of all injured portions of bone, and with them the most important factors of trouble after such an injury, thus permitting rapid healing, and the smallest possible amount of inflammatory adhesions between muscles, tendons, etc. If the bone had been simply returned, the risk to life would have been greater, owing to the prolonged suppuration incident upon the separation of the necrosed bone and the deep-seated abscesses so common after compound fractures. Against it was the absolute shortening of the arm, with the prospective cessation of growth due to removal of the upper humeral epiphyses.

The actual result, I think, bears me out in the course of treatment pursued, for I hardly think that in seven weeks he would have been in so good a condition, with the wound soundly healed, if I had followed what is often, but falsely, called the "conservative" plan of treatment. I believe that true conservatism indicated exactly what I did. The amount of shortening would not have been much less had the case been left to nature and necrosis. Had this occurred, union of the severed head could not have taken place; and then the same shortening would have obtained as surely as if the epiphysis had been removed. Army experience has shown that when a portion of the upper end of the humerus is removed for injury, nothing is gained by leaving the uninjured head, since it necroses.

Although not cognizant of this fact of experience at the time of operation, anatomical knowledge, general surgical principles and experience induced me to arrive at a conclusion by *a priori* reasoning which I have since found that extended experience had already proved.

I believe, therefore, that, theoretically and from experience, resection ought to be performed for such injuries. It is hardly necessary to say anything about the operation itself, since each case must be a rule for itself, the only point being to remove the bones with as little additional damage to the soft parts as practicable. The wound was dressed antiseptically, and when I transferred the wards to my colleague, Dr. Packard, no suppuration had occurred, and there was not the slightest inflammatory blush about the wound. He did uninterruptedly well, and the wound was soundly healed in less than seven weeks, the greater part at a much earlier date.

Sharply contrasted with this case and its results is that of the patient from whom the next specimen was removed, where the head of the humerus, luxated and partially fractured and protruding through the skin of the axilla, was *reduced* instead of being resected. Here the tension of irritated lacerated muscles, conjoined with the necessarily imperfect drainage, kept the injured bone bathed in unhealthy pus. This, with the original injury, resulted in an osteomyelitis, which necessitated my amputating at the shoulder-joint. I believe, had the head of the bone been removed, a fairly useful limb would have been the result at the end of a few weeks' treatment, while instead, after three months of illness and risk to life, amputation was the best I could do for him.

Compound Luxation (with Fracture) of the Shoulder-Joint.

— —, æt. 30 years, man, three months ago had his right arm caught by the belting and drawn over a large drum in a position of extreme abduction and probably of extension. The head of the bone was luxated, the greater tuberosity torn off, and the caput humeri thrust through the axillary integuments near the anterior axillary fold. When I first saw him at the Episcopal Hospital after the accident he was very pale, with a constant discharge of pus from an opening at the site of the old wound, *i. e.*, near the anterior axillary fold, while the orifice of another deep-seated sinus was seen over the middle of the triceps on the outer side of the arm. A probe introduced into the anterior sinus readily touched the denuded carious head of the humerus. I attempted to exsect the head of the bone, but when prepared to saw it, after its protrusion through the wound, I found such evidences of osteomyelitis as to render amputation at the shoulder-joint necessary. He did well and recovered, but even some months later a sinus existed, doubtless the result of necrosis of some of the fragments of periosteal bone, produced by that irritated structure. As before said, had the head of the injured bone been excised, a useful arm would have probably resulted.

The third and last case is one where a comparatively trivial injury, owing to non-treatment at first, resulted in a condition which demanded resection of the shoulder-joint.

Chronic Arthritis of the Shoulder-Joint: Epiphyseal Abscess of the Humerus.

Anna M., æt. 17 years, was admitted to the Female Surgical Ward of the Episcopal Hospital, May 14, 1883. One year ago last May she fell down stairs and struck her shoulder. She was unconscious for a short time, but was soon able to walk home. The arm did not become inflamed, and seemed to the patient well. Nine months after the fall she noticed pain in

the shoulder, and an elevated papule formed near the joint, which was opened at the dispensary. This relieved the pain, but left a fistulous tract discharging healthy pus. She attended the dispensary until the 14th of last May, when she was sent into the house, Dr. Seltzer, the Assistant Surgeon on duty, having touched dead bone with the probe. After admission she had pain from time to time, gradually increasing in intensity until shortly before operation. Other free openings for drainage were made by Drs. Simes and Kelley. The probe detected an apparent sequestrum within the humeral head. Diagnosis, epiphyseal abscess.

The operation showed complete destruction of the joint, a carious and denuded humeral head with an abscess about the epiphyseal site containing a sequestrum. The glenoid cavity was denuded of cartilage and roughened. The portions of head and shaft, such as you see, were removed, while all the glenoid cavity was cut away with the gouge-forceps, except where the long head of the biceps was attached. Further details of the operation are unnecessary. The patient was practically well at the end of two weeks. Perfect quietude of the joint at the outset might have averted all subsequent trouble.

What was the condition here after the accident? Probably the bursæ and fibrous tissues surrounding the joint were involved, and the vascular epiphyseal cartilage was congested from the jar and injury of the fall. Congestion of all these parts, instead of being relieved by complete functional rest of the articulation, with the local application of ice, leeches, etc., as appeared indicated, was kept up by the girl following her usual occupation of housework. Although in no sense markedly strumous, yet the tendency was in that direction. As the congestion increased, inflammation and suppuration were set up in the bursæ, the disease spread to the articulation, gelatinous arthritis with epiphyseal abscess supervened, notwithstanding the skilful treatment of my colleagues who, *too late*, had the opportunity of treating the case.

NOTE ON A SPECIMEN OF INTRA-CAPSULAR FRACTURE OF FEMUR WITHOUT UNION.

Read March 19, 1884.

BY HENRY LEAMAN, M. D.

MRS. MARY A. NUGENT, aged 60 years, fell January 23, 1881, while attending her household duties. Was not dizzy at the time.

I saw her first January 24, 1881; she was then lying on a settee and was unable to stand or move about. On examination, from the preternatural mobility intra-capsular fracture was diagnosed. My attendance was continued at intervals during the months of February, March, April and May. She was unable to help herself in any way; by help she could be gotten on a Charleston chair, and this was the only treatment attempted. The most of her pain was in the adductors; there were shortening and eversion of toe.

She continued in the same room and on the same settee and chair until I was called to see her, January 6, 1884, when I found her dying from exhaustion. She died January 8, 1884. Privilege was granted to examine the hip. The specimen here presented was obtained. The acetabular cavity was normal in appearance. The head of the femur lay in the cavity free. The socket and head of the bone were covered over with a membrane, firm but somewhat incomplete. Upon this surface the upper extremity of the femur had formed an artificial joint, the neck having been absorbed. In the surrounding tissue of the inner side was a fragment of bone one and one-half inches long.

This case has no special interest, except, perhaps, in the fact that the head had undergone fatty degeneration, and that the patient made so little progress towards locomotion, never being able to walk on crutches. Doubtless degeneration in other parts of the body caused her death. But no further post-mortem was granted.

DISCUSSION ON INTRA-CAPSULAR FRACTURE.

DR. CLEEMANN: I witnessed the post-mortem in a case of a man, more than six feet in height, who had an unrecognized fracture of the neck of the femur. The neck of the bone had entirely disappeared, and in the socket was a loose flattened ball, the remains of the head of the bone. I have been able to detect by mere inspection the existence of a fracture

of the neck of the femur. The shortening of the limb causes a wrinkle or crease in the inelastic ligamentous tissue beneath the patella, which may be very strongly marked in an old person in whom the pad of fat normally existing under the ligament has been absorbed. I have, however, seen the "crease" in a vigorous man of 25 years of age.

DR. NANCREDE: I have been unable to satisfy myself of the usefulness of the symptom pointed out by Dr. Cleemann. I wish to condemn emphatically the violent jerking, pulling and twisting to which an intra capsular fracture is so commonly subjected to determine the presence of crepitus. In an elderly patient, say one about 60 years of age—even one over 50 years oftentimes—in whom there is distinct shortening of the limb, etc., with entire absence of the symptoms of luxation, I do not worry myself much about eliciting crepitus, but prefer to treat the case as one of intra-capsular fracture. If the patient's health seems to suffer from confinement to bed, I would be inclined to sacrifice the problematical chance of an improved limb to the salvation of the patient's life.

DR. LEAMAN: I coincide with what Dr. Nancrede has said. I am much opposed to such violent treatment of cases as is sometimes done to aid in making a diagnosis. In the case in question, violent means were not used.

MALIGNANT PUSTULE.

Read April 9, 1884.

BY W. S. JANNEY, M. D.

MY remarks to-night will be on the clinical history of four cases of malignant pustule, which have come under my observation at various times during thirty years of practice.

The synonyms of malignant pustule, as given by various authors, are: Contagious carbuncle, malignant carbuncle, anthrax and charbon; other names are used to designate the more diffused and general forms of the disease.

It is defined to be a specific contagious disease, communicated to man from disease of horned cattle, horses, sheep, and other herbivora, and known as splenic fever, and due to the presence in the system of the bacillus anthracis of Cohen, or bacteridium of Davaine. The local or external form of the affection, malignant pustule proper, is a carbunculous swelling having specific characters, attended with more or less intense surrounding inflammatory cedema; constitutional symptoms may be slight or severe, and the disease is often fatal.

The symptoms and course of malignant pustule vary greatly

with the form of disease. Authors describe at least three distinct forms :

First : Malignant pustule or carbuncle proper, the form from which the names of charbon and anthrax are derived ; usually it occurs as a primary lesion due to direct inoculation ; the seat is either on the face, neck, hands or arms, those parts most exposed to inoculation.

Second : Malignant anthrax, œdema, without definite pustule, corresponds in the main with malignant pustule proper. The eyelids are the parts most frequently affected, but it may occur elsewhere.

Third : Internal anthrax ; differs greatly from external, and may be general, having no special lesion or accompanied by local affection ; usually pulmonary or gastro-intestinal.

The cases that I wish to report to-night come under the form of malignant pustule proper.

Mr. H., residing in Hopewell township, Mercer county, N. J., a farmer, aged 60 ; previous to the attack general health good. On the morning of September 20, 1866, Mr. H. noticed a small pimple on his right cheek, immediately over the infra-orbital foramen. During the day it was slightly painful, and during the night the apex became vesicular, with great itching and burning, which continued to increase until the following morning, when I first saw him. The face presented the following appearance : a small pustule, one-eighth inch in diameter, situated as above stated, with a denuded apex of a dark brown color, and an areola of one-half inch in diameter of a dark red color, surrounding the base of the pustule, and not sensitive to touch ; pulse 78, respiration 20, tongue slightly coated and bowels constipated. During the day the temperature increased. Pulse in the evening 100, respiration 22. Side of face up to this time had become very much swollen, with red streaks extending to the neck ; slightly delirious ; tongue dry. Free crucial incisions were made in the pustule. Delirious through the night of the 21st. On the morning of the 23d, respiration was 30, pulse 180, tongue brown and dry ; the cheek of dark gangrenous color, extending to the lower margin of lower jaw and also backward, involving the parotid region and right ear, to near the posterior median line of the neck. Dark red streaks extending over the shoulder to the right arm ; patient becoming rapidly comatose and died at 2 P. M. on the 23d ; being fifty-eight hours after first noticing pimple on his face. The inflammation and œdema did not extend over the median line of the face or back of the neck.

The second case that came under my care was :—

Mrs. R., residing at her country seat in the suburbs of this city ; 33

years of age; married; the mother of three healthy children, and of previous good health. She noticed, September 17, 1876, a small pimple on the right side of the face, one-half inch below the lower lip, and slightly to the right of the median line. From her description of how it commenced, she informed me that her first intimation of anything being the matter was a persistent itching sensation, and on rubbing it she felt a small circumscribed induration, which was in the skin, and was not noticeable. She continued rubbing it to allay the itching; in a few hours she noticed a slight elevation of the skin, conical in form, and the size of an ordinary pin-head, which increased during the day. Slept well during the night, and on the following morning, the 18th, she noticed the papule had increased in size, and was vesicular, containing a dark colored fluid. The itching continued, and on rubbing it she ruptured the vesicle, and from that time she had a burning, itching pain in the pustule. I saw Mrs. R. on the morning of the 18th of September, she was sitting in her room and did not consider herself sick; had slight headache; was nervous, and spoke of a premonition of impending sickness or calamity. She had an anxious expression; retraction of the eyelids, giving her a staring expression. Tongue slightly coated with a light yellow coat; temperature 99, respiration normal, pulse 80; constipated, and urine scanty; on her face, half inch below the right side of the lower lip, and half inch to the right of the median line, was a pustule of the size of a split pea, with an indurated base half an inch in diameter, of a dark red color. The apex of the pustule was denuded of cuticle, and of a dark brown color, not sensitive to pressure; no lines or streaks of inflammation extending from the pustule; no œdema of face. On the evening of the 18th temperature was 100, pulse 110, respiration 22; tongue coated and dry; streaks or lines of a dark red color, extending from the right side of pustule in a line of the inferior maxilla, curving upwards towards the right ear; right half of lower lip swollen, and of a dark red color, which, on pressure, imparted a nodulated condition; dark red streaks extending from the lip, curving upwards to the integument over the malar process. The skin and underlying tissues between the base of the pustule and the indurated lip, retained their normal color and consistence.

Complained of severe lancinating pain over right half of face and shoulder. Morning of the 19th, temperature $101\frac{1}{4}$, pulse 124, respiration 24; tongue dry; sordes on teeth; the areola around the pustule not so red; no discharge from pustule; dry and dark in color. The lower right half of lip showing dark gangrenous patches; right half of upper lip swollen, of a dark purple color, hard and nodulated. The right half of the face, forehead and right ear swollen, œdematous, and of a mahogany color. The right side of neck swollen, with red streaks extending to the shoulder and arm. Complains of lancinating pains in right arm, forearm and hand; also of scalp.

I saw her again on the evening of the 19th, when all of the above symptoms were aggravated. The right half of the face and right half of forehead, scalp, neck and arm presented the appearance of rapid exten-

sion of gangrene. The lower half of right lip completely gangrenous; upper lip also. The fauces, tonsils and pharynx not affected; lancinating pains in right mammary region, abdomen, and lower extremities. There was no redness, cedema, or other indications of the disease extending to the mammary region, abdomen, or right lower extremity. Skin normal in color; the slightest touch of the integument over the right side of the thorax, thigh and leg produced the most excruciating pain, and not upon the left; became comatose during the night of the 19th. On the morning of the 20th, gangrene had extended during the night to the shoulder and arm, as far as the elbow, and to the median line of the neck posteriorly; had stertorous breathing, temperature 108, respiration 30; died at 12 o'clock, 72 hours after she had first noticed the papule.

The third case:—

Mrs. H., residing in this city; was called to see her October 20, 1878; 30 years of age, and of previous good health; mother of five children. Found her dying. She had been under the treatment of another physician. The history of the case was obtained from her husband; four days previous to her death she noticed a small papule on the right side of her chin; on the following night the lower lip began to swell, extending to the median line, and next day involving the right half of the upper lip and extending over the left side of the face; complained of lancinating pain over right side of face, head and neck; was five months pregnant, aborted on the third day, became comatose on the night of the third day, and died on the morning of the fourth day. When I saw the case, the lower and upper right half of the lips were gangrenous; between the pustule and lower lip an area of healthy tissue intervened, similar to case second.

The fourth case occurred in this city:—

Mr. P., residing at 2140 Park Avenue, a wool merchant, set. 24, and of previous good health, who had been in Colorado, purchasing wool, returned from Colorado, October, consulted me October 3, 1888, for a cough, the result of a cold. On examination I observed on the face, one inch below the right half of the lower lip, near the median line, a small papule, not larger than a small pea, with an areola half an inch in diameter, of a pale pink color; I directed his attention to it, and he remarked that it was nothing but an ordinary pimple. My experience with the cases reported led me to suspect that it might be the beginning of a malignant pustule. As he had been handling wool in Colorado, I stated my suspicions and asked him to call next morning; incised the papule and applied a fly-blistar. He attended to his usual business on the 3d of October, and called at my office on the morning of the 4th; the papule had not increased in size. The areola was of a much darker color, but not increased in area. I removed the vesicated skin from the papule and applied another blister; had headache, temperature 99 and respiration 20. I felt almost convinced that I had to encounter another case of this dreadful disease. By much persuasion he

permitted me to incise the pustule freely; was requested to go home, and told that I would see him in the evening.

On the evening of the 4th, temperature 100, pulse 95, respiration 20; no perceptible change in the pustule; red lines extending outward from left side of pustule, curving upwards over the face; lower right half of lip swollen and hard, with a band of hardened tissue extending from the left angle of the mouth outward for two inches; the face œdematous.

Incised the lower lip transversely from the median line to the angle, on the line of junction of the skin and mucous membrane, to the depth of one inch, and applied pure carbolic acid to the wound; also injected pure carbolic acid into the pustule; applied a poultice of flaxseed, tar and tinct. iodine—3 parts of meal, 1 part of tar, 2 dr. of tinct. iodine.

Morning of the 5th, inflammation around pustule less; lower lip more swollen and presenting a gangrenous slough; left half of upper lip swollen and presenting the same appearance as lower lip twelve hours previous. Temperature 101, pulse 115, respiration 22; face more œdematous, and dark red lines extending from the lips upwards and backwards to the zygoma and left orbit. I incised upper lip and applied carbolic acid, and poultice; injected carbolic acid into the tissues near the angle of the mouth; applied lint wet with sol. act. lead to the face. Evening of the 5th, temperature 102½, pulse 124, respiration 24; the œdema of the face has not extended beyond the limits in the morning; tissues of upper lip of darker color; lower lip sloughing; pustule and surrounding areola improving in color; slight discharge of pus from pustule.

Morning of the 6th, passed a very restless night. Temperature 102, pulse 120, respiration 20. Tissues of lower and upper lip sloughing; removed with forceps and scissors a great portion of the slough of the lower lip; continued to apply carbolic acid. The œdema and color of the face remained in much the same condition of previous day. Evening of the 6th, temperature 103, pulse 130, respiration 24. No perceptible change in the pustule, lips or face since morning.

Morning of the 7th, temperature 102½, pulse 128, respiration 22, œdema of face diminished, pustule and lips discharging pus. Evening of the 7th, temperature 103½, pulse 135, respiration 26; has been chilly during the day, and is in a profuse sweat at 6 P. M.

Morning of the 8th, temperature 102½, pulse 126, respiration 22; sloughing of upper lip profuse. Evening, temperature 104, pulse 140, respiration 30.

Morning of the 9th, temperature 103, pulse 138, respiration 28; entire slough of lower lip removed, presenting a healthy granulating surface. Upper lip sloughing; removed from angle of mouth a large slough; face less swollen and less discoloration. Slightly delirious during the previous night. Evening, temperature 105, pulse 142, respiration 30.

Morning of the 10th, temperature 102, pulse 130, respiration 28. Night, temperature 103, pulse 130, respiration 34. Condition of face improved, slough removed from upper lip.

Morning of the 11th, passed a restless night; had slight chill followed by a profuse perspiration; temperature 103½, pulse 140, respiration 28; tongue

dry and sordes on teeth ; bowels loose ; redness and œdema of face rapidly disappearing ; the lips presenting healthy granulating surfaces ; swelling and fluctuations below the symphysis of lower jaw ; punctured, and half oz. of pus evacuated. Evening, temperature $105\frac{1}{2}$, pulse 130, respiration 30.

Morning of the 12th, temperature 102, pulse 130, respiration 28 ; profuse perspiration through the previous night. Evening, temperature $103\frac{3}{4}$, pulse 140, respiration 28.

Morning of the 13th, temperature 100, pulse 106, respiration 24. Swelling with fluctuation over infra-orbital foramen. Punctured, and evacuated one oz. of pus. Evening, temperature 103, pulse 140, respiration 30.

Morning of the 14th, temperature 99, pulse 110, respiration 22. Profuse perspiration, alternating with chilliness during the previous night and day, and complains of pain and soreness of right leg. On examination found an area of dark red color, one inch wide and two inches long, situated on the outside of the anterior border of the tibia, at the junction of the middle with the upper third of the bone. Introduced bistoury to the depth of one inch and a half, without reaching pus.

Morning of the 15th, temperature $100\frac{1}{4}$, pulse 132, respiration 24 ; passed an uncomfortable night ; had profuse perspiration ; wounds of lips improving. Evening, temperature 104, pulse 140, respiration 32.

Morning of the 16th, temperature 103, pulse 140, respiration 30. Severe chill during the night, followed by severe lancinating pain in lower right pleura. The swelling in the leg continued, and a deeper incision extending between the tibia and fibula, giving exit to three ounces of dark-colored pus. Evening, temperature 105, pulse 160, respiration 36.

Morning of the 17th, temperature $102\frac{2}{10}$, pulse 128, respiration 30 ; had alternate chilliness and perspiration during the night. The acute pain in the side relieved, with a dull aching pain ensuing ; slight cough on full inspiration. Percussion revealed dullness over the lower lobe of right lung. Evening, temperature $104\frac{3}{4}$, pulse 140, respiration 36.

Morning of the 18th, temperature $104\frac{1}{4}$, pulse 128, respiration 30 ; expectorates frothy mucus, tinged with blood ; continues to have profuse perspiration several times a day, so that his clothing is continually wet.

Morning of the 19th, temperature 101, pulse 126, respiration 28. Expectoration of bloody sputa increased. Perspiration continuing. Abscess in leg discharging unhealthy dark-colored pus. Evening, temperature 103, pulse 132, respiration 32.

Morning of the 20th, temperature 101, pulse 124, respiration 28. Expectoration of a dark brown color. Less dullness on percussion. Abscess still discharging pus of a lighter color. Evening, temperature $103\frac{3}{4}$, pulse 132, respiration 30.

Morning of the 21st, temperature 100, pulse 120, respiration 22. Expectoration less and of lighter color. Urine examined ; quantity, 30 oz. daily, and slightly albuminous. Evening, temperature $102\frac{3}{4}$, pulse 128, respiration 26.

Morning of the 22d, temperature 101, pulse 124, respiration 26. Expec-

toration less. Less dulness over lung. Evening, temperature 103, pulse 130, respiration 30.

Morning of the 23d, temperature 99, pulse 120, respiration 24. Evening, temperature 102½, pulse 130, respiration 28.

Morning of the 24th, temperature 101, pulse 130, respiration 22. Very little change in the patient's condition for the last two days. Evening, temperature 103½, pulse 134, respiration 28.

Morning of the 25th, temperature 99, pulse 120, respiration 22. More air entering right lung. Less cough and expectoration. Otherwise no improvement. Evening, temperature 102½, pulse 128, respiration 30.

Morning of the 26th, temperature 99½, pulse 120, respiration 22. Evening, temperature 105, pulse 136, respiration 30.

Morning of the 27th, temperature 101½, pulse 122, respiration 22. Evening, temperature 102½, pulse 130, respiration 28.

Morning of the 28th, temperature 99½, pulse 124, respiration 24. Less cough and expectoration, and profuse perspiration at intervals of four to six hours. Evening, temperature 103, pulse 132, respiration 30.

Morning of the 29th, temperature 99½, pulse 120, respiration 22. With the exception of temperature, the patient appears to be improving. Evening, temperature 103, pulse 130, respiration 24.

Morning of the 30th, temperature 99, pulse 122, respiration 20. Evening, temperature 103, pulse 128, respiration 26.

Morning of the 31st, temperature 99½, pulse 120, respiration 20. Evening, temperature 102½, pulse 124, respiration 24.

Nov.

10th day, . . .	Morning temperature	102,	Evening	102.
11th " . . .	"	"	101½,	" 102.
12th " . . .	"	"	101,	" 101½.
13th " . . .	"	"	101,	" 100.
14th " . . .	"	"	100,	" 100½.
15th " . . .	"	"	100,	" 100½.
16th " . . .	"	"	99,	" 101½.
17th " . . .	"	"	100,	" 101.
18th " . . .	"	"	100,	" 101½.
19th " . . .	"	"	99½,	" 101.
20th " . . .	"	"	99,	" 100.
21st " . . .	"	"	99,	" 100½.
22d " . . .	"	"	100,	" 100.
23d " . . .	"	"	99,	" 100.
24th " . . .	"	"	98½,	" 99½.
25th " . . .	"	"	99,	" 100.
26th " . . .	"	"	99,	" 100½.
27th " . . .	"	"	98½,	" 100.
28th " . . .	"	"	98½,	" 100½.
29th " . . .	"	"	98½,	" 100½.
30th " . . .	"	"	98½,	" 100.

Dec.			
1st day,	. .	Morning temperature	98½, Evening 99½.
2d	" . .	"	99, " 99½.
3d	" . .	"	98½, " 99½.
4th	" . .	"	99½, " 100½,
5th	" . .	"	99, " 100½.
6th	" . .	"	98, " 100.
7th	" . .	"	99, " 100.
8th	" . .	"	99, " 100.
9th	" . .	"	98, " 99½.
10th	" . .	"	98, " 99.
11th	" . .	"	98, " 99½.
12th	" . .	"	98, " 99.
13th	" . .	"	98, " 99.
14th	" . .	"	98½, " 99.
15th	" . .	"	97½, " 99.
16th	" . .	"	98½, " 99.
17th	" . .	"	97½, " 99.
18th	" . .	"	97½, " 99.
19th	" . .	"	97½, " 99.
20th	" . .	"	97½, " 99.
21st	" . .	"	98, " 99½.
22d	" . .	"	98, " 100½.
23d	" . .	"	98, " 99.
24th	" . .	"	97, " 99.
25th	" . .	"	98, " 99.
26th	" . .	"	98½, " 99.
27th	" . .	"	98½, " 99.
28th	" . .	"	96½, " 98½.

On the night of November 1st he had a severe chill, after which the temperature rose to 106, followed by severe pain in left thorax, which proved to be the beginning of another attack of pleuro-pneumonia, which passed through all the stages that I have just related in the attack on the right side, with temperature, pulse and respiration during the course of the disease a counterpart of the first attack.

On the morning of the 10th of November, temperature 102½, pulse 130, respiration 28. Patient continued to improve from this date. A slight cough with expectoration of light-colored sputa continued until December 28th, with occasional attacks of perspiration; the temperature was taken on until the 28th of December.

The characteristic symptoms of two of these cases were alike in several respects. The locations of the pustules were both on the right side of the face, and located at the same place. The intermediate integument between the pustules and lips were not affected by the disease in either case. The inflammation or exten-

sion of the disease appeared to be from the right side of the pustules along the integument covering the basilar portion of the inferior maxilla, to near the angle of the jaw, and then curving upwards over the face along the anterior border of the Masseter muscle.

On the second night, or twenty-four hours after pustules were noticed, and twelve hours after red streaks or lines extended along the lower margin of jaw, and then the lower half of the lips became affected, and twelve hours after the upper lip became affected in both cases, and then the right half of the face, forehead and scalp in the case of Mrs. R., and the face of Mr. R. became œdematous.

From the observation of these cases it appears that the disease may be divided into four periods or stages—first, the period of incubation, which may be from a few hours to fourteen days, with no prodones; second period, the formation of pimple, papule, and pustule, lasting from twelve to twenty-four hours; third stage, the extension of the œdema and inflammation, occurring twelve hours after the formation of the pustule; fourth, the stage of gangrene, occurring in from twelve to twenty-four hours later. The disease extended by the poison being carried by the superficial lymphatics only. I am led to this conclusion from the fact that in three of the cases the disease extended from the right side of the pustule, curving upwards over the face; and not until the lines of inflammation or œdema had reached above the line of Wharton's duct, did the lips show evidence of disease. Again, the disease in all of the cases was confined to one side of the face, head, neck and scalp, and did not pass over the median line of the face or the median line of neck posteriorly. The treatment of all of the cases was similar in most respects.

In the second case, Mrs. R., the treatment was free crucial incision of the pustule; injection of pure carbolic acid into the pustule; quinia in large doses, carbonate ammonia, tinct. ferri chloridii and whisky punch internally; free incision of the lips, and injection of pure carbolic acid, with local application of alcohol to the face.

The third case, Mrs. H., I did not treat.

The fourth case, Mr. P., was under my care from the time the papule was formed; free crucial incision was practiced at once, and pure carbolic acid was injected into the tissues around the

pustule; he was put upon quinia, four grs. every three hours; tinct. ferri chloridii, thirty drops every three hours, and whisky punch. As soon as the lips showed indications of the disease, free incisions were made, and carbolic acid was injected into them, and also into the angle of the mouth; lead-water and laudanum applied to the face, which appeared to act better than alcohol; used as a poultice, linseed meal, tar, and tinct. iodine; when indications of septic poisoning occurred, he was given aqua chlorinata in drachm doses every four hours, which was continued until December 20th. The attacks of pleuro-pneumonia were treated by counter-irritation of the thorax, and quinia, carbonate of ammonia, with the addition of morphine.

The immediate cause of death in these three cases was, I believe, by thrombus of the cerebral veins or sinuses, the intimate connection of the pterygoid plexus with the facial vein, also the connection of the ophthalmic vein with the angular vein; a continuation of the facial, and the vein passing from the internal surface of the nasal cavities up through the foramen cæcum to the longitudinal sinus; the pterygoid veins and ophthalmic veins emptying into the cavernous sinus. Mr. H., Mr. R. and Mrs. H. became rapidly comatose, had stertorous breathing and complete paralysis before death, all symptoms of compression of the brain. Bilroth reports a case of death from malignant pustule, in which the post-mortem examination showed thrombus of the temporal veins, that was traced to the ophthalmic, and through the ophthalmic to the brain. Bartholow gives as the most frequent cause of sudden death in erysipelas of the face and head, thrombus of either the longitudinal, cavernous, or lateral sinus.

"In cases of malignant pustules rigor mortis usually sets in early, and passes off quickly; the body is often cyanosed; the face may be swollen; petechiæ on chest and abdomen are not uncommon; decomposition usually sets in early. The blood is generally dark, lake and tarry, and in the heart often uncoagulated; the subcutaneous cellular tissue of the parts affected is hæmorrhagic, and hæmorrhagic patches radiate into the surrounding tissues, which are extensively infiltrated with a semi-gelatinous blood-stained fluid. In the pulmonary and gastro-intestinal form, other anatomical characters are observed.

"The most important point in the microscopic anatomy is the presence of the bacillus anthracis in the blood and tissues, either

diffused or forming masses in the lymphatics and vessels; the bacillus anthracis, as seen in the blood, consists of a motionless, short, apparently homogeneous rod or filament, rarely less than $\frac{1}{1000}$ of an inch long, either straight, curved or bent at an acute angle. The usual mode of multiplication in the blood is by transverse fission. The bacillus anthracis requires for its growth the presence of a nitrogenized pabulum and a supply of oxygen; its vitality is destroyed by a temperature of 60° C.; when dry the rods themselves can be preserved but a short time, while the spores retain their vitality for years and are unaffected by ordinary changes of climate or temperature."—GREENFIELD.

The bacillus anthracis is a bacterium, first discovered by Pollender, in 1849. All parts of the bodies of animals dying of the disease are actively poisonous, and may convey the disease by direct or mediate contagion; it may arise from eating the flesh, though the poison is said to be destroyed by cooking; contagion may also be conveyed by butter or milk. The bites of flies may also convey the poison. Contagion occurs in those who have to deal with the wool or hair of animals which have died of the disease, such as wool packers and sorters, horse-hair cleaners, furriers, tanners. The poison may enter the system either by local inoculation, or by inhalation of the dust containing it. The diffusion of the poison by water, and its distribution by means of wool-waste and bone-dust, used as manure, especially deserve notice, as capable of spreading the contagion.

"In the earlier stages diagnosis is very difficult, except in persons who are known to be exposed to contagion. At a later stage the characteristic features of the pustule render the recognition comparatively easy, and microscopical examination of the serum contained in the vesicles shows the presence of the bacillus anthracis. Inoculation experiments on guinea-pigs or mice will, if successful, readily decide it, but no absolute conclusion can be drawn from failure to inoculate.

"The prognosis is extremely unfavorable."

DISCUSSION ON MALIGNANT PUSTULE.

DR. TYSON: I regret that I can add nothing from personal experience in this interesting disease. It is to be regretted that there was no microscopic study of the blood, with a view to determining the presence of the bacillus anthracis. For although there appears to be less doubt as to the

causative relation of the bacillus of this disease than of any other, it is still important that observations should be multiplied in this country, most of the investigations having been made abroad, in France and Germany.

DR. SHAKESPEARE: It would have been very interesting to have noted the microscopical appearance in these cases, and to have proved the diagnosis by inoculation. The author of the paper admits the bacillus anthracis as a cause, or at least as a concomitant of the disease. I cannot agree with him as to the inference from failure to inoculate the lower animals. It is known that these may resist infection, either from protection by previous attacks, or from special idiosyncrasies. But such protection can scarcely be claimed to exist when the disease does not exist, and anthracis is extremely rare among the cattle of this country. By inoculation in some animals, such as the house-mouse, we can nearly always make a diagnosis in the earlier stages, and if resort be had to the microscope, the diagnosis is nearly certain. I paid considerable attention to the subject of anthrax in the lower animals during my recent visit to Koch's laboratory. It is worthy of remark that the field-mouse, although much like the house-mouse, and some species of sheep, seem to resist well the infection. We are principally indebted to French observers for our knowledge on this matter. I had no difficulty in isolating, cultivating, and inoculating the anthrax bacillus. The animals died promptly, and their blood-vessels were found obstructed, and in the lungs absolutely plugged by the typical bacilli, arranged in twisted filaments. The cases in the human subject are rare in this country, and it is remarkable that four should have occurred in the practice of one surgeon in so short a time. The occurrence of this and other disease in man, as an infection from animals, shows the great importance of scientific study of the diseases of these animals, a matter which in this country has not yet received adequate consideration.

DR. WM. H. PANCOAST: The contagious character of malignant pustule is said to be due to the presence of the bacillus anthracis or bacterium, which is said to have just been discovered by Pollender, in 1849. Duraine gave it the name of bacterium, which is generally used in France; Cohn, of Germany, employing the term of bacillus anthracis.

I was present some years ago at a meeting of the Academy of Medicine, in Paris, during a heated discussion of this subject. Pasteur, I believe, was present, and took part in the debate. It is in print that Pasteur states that the bacillus may be developed in the earth surrounding buried carcasses, and may be thus cultivated—that the worms in the earth may work to the surface and distribute the bacillus to the adjacent vegetation. If this is a fact, it may explain the origin of the cases described by Dr. Janney occurring among farmers.

I have never seen a case of malignant pustule in my own practice; but several years ago I saw one in the service of my father, in the Pennsylvania Hospital. The patient was a knacker from "the neck," and had acquired the contagion in skinning some dead cattle. The pustule was upon the

right arm, if I recollect correctly. In spite of every effort the patient died from the poison.

I am impressed with the value of the hot iron in such cases—making free incisions, and pushing the iron at a white heat into them and into the wound, so as to destroy the bacillus. The hot iron is a king attractive, and one of the very best. It not only destroys the part it touches, but the heat radiates beyond and effects molecular change in the deeper parts. Carbolic acid does not penetrate so deeply.

I think it is important to open the pustule freely, and to tap with a fine-bladed knife the surrounding parts of swollen and inflamed tissue; to use what I call the antiphlogistic touch of the knife—not simple or multiple incisions, but numerous punctures, as many as may be required to relieve the inflammation or let out the congested blood. I find this method very useful in many chronic and acute inflammatory congestions.

In opening an ordinary carbuncle, one non-malignant, I also think it is important to incise freely. In an ordinary boil there are only a few masses of cellular tissue inflamed, and a simple straight incision from without inwards, and out again, will open them all; or numerous punctures with the antiphlogistic use of the knife. But in a carbuncle there are so many packets of inflamed cellular tissue, that a simple linear incision and even a crucial one will not open them all. I therefore make one free incision through the (carbuncle) inflamed mass, and then, with the same curved bistoury, incise freely, and in every direction subcutaneously, the sides of the inflamed mass on each side of the straight incision. By this method, with only one incision, I open freely and penetrate the knife deeply into all the inflamed tissues, making the greatest antiphlogistic impression, and yet leave only one straight scar.

(Dr. Pancoast asked Dr. Janney if there were any anatomical post-mortem characteristics; if the rigor mortis had set in early; if the body was cyanosed, or if there was much hypstatic congestion or extravasations of blood.

DR. JANNEY answered that there was no post-mortem examination made.)

DR. S. ASHHURST: I recall the case referred to by Dr. Pancoast, as occurring in the practice of his father, Dr. Joseph Pancoast. He used, however, a more energetic means than the hot iron, viz., a strong solution of zinc chloride. For my own part, I do not like the hot iron.

DR. STONE: I wish to ask Dr. Janney if he has any theory to account for the favorable result obtained in the treatment of one of the cases.

DR. PANCOAST: Would inoculation of the lower animals show itself sufficiently early to be of use in diagnosis?

DR. SHAKESPEARE: Small animals, such as mice, die in twenty-four hours after inoculation.

DR. TYSON: Is the appearance of the pustule an evidence of the general involvement of the system, and, if so, what advantage is to be gained by treatment of the local manifestation by cauterization or excision?

DR. SHAKESPEARE: It is true that after the disease has reached the general system, cauterization of the local manifestation, except in so far as the latter constitutes a depot for the continued production of the disease germs which may still be added to those already in the system at large, is useless.

DR. JANNEY, in closing the discussion said: I think that many cases of malignant pustule occur in this city, and are not reported. I cannot be absolutely positive that my cases were malignant pustule, because no microscopic examination was made. I judge from the character and clinical history. In three of the cases no cause could be traced; in the fourth there was only the supposition of cause, since the patient had been among conditions that might be a source of infection. Dr. Welsh saw one of the cases and thought it was malignant pustule; Dr. D. Hayes Agnew saw the fourth and pronounced it the disease.

As regards treatment, in one of the cases it was evident that no local treatment would avail. I have a theory that in the favorable case the early incision and the injection of carbolic acid in the lips destroyed the bacillus germs, which had as yet not extended to the cheek. The remedy that seems of greatest value in this disease, as indeed in other forms of blood-poisoning, is chlorine-water.

After the debate, DR. PANCOAST stated that he liked chloride of zinc as an alterative application, having learned its value from his father's practice; that he used it on chancres, cutting them out when possible, and applying it on the raw surface, and also upon morbid growths. But in the bites of dogs he also used the hot iron for its radiating effect; and while he might use it as an application to malignant pustule, yet he would also use the hot iron, pushing it into the incisions.

DR. SHAKESPEARE regarded the affection more commonly called anthrax as a very different thing from malignant pustule. It is the latter which has the same cause as anthrax in the lower animals. The remarks of Dr. Pancoast concerning carbuncle recalled his intention to say that the incandescent iron is a more proper remedy for malignant pustule. It is our most effective bactericide. Carbolic acid may not penetrate nor destroy even when we use the concentrated solution, for this becomes dilute by contact with the tissues. The bacillus of anthrax often contains spores; these will be propagated from pus and cannot be killed by dilute carbolic acid. It appears that the most efficient chemical agent for the purpose is a strong solution ($\frac{1}{100}$) of corrosive sublimate. But since it is a question of life or death of the patient, with the probabilities against him, he thought the surgeon should not be influenced by cosmetic considerations, but should promptly and with a free hand apply the most certain means of killing the bacillus while it is brooding, and before it has spread beyond the pustule.

ON PAROXYSMAL FEVER—NOT MALARIAL.

Read March 26, 1884.

BY J. H. MUSSER, M. D.

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THAT non-malarial intermitting fever is of frequent occurrence few will deny. Such cases have come to the writer's notice so often, that, especially as but little can be found in reference to this subject in medical literature, arranged in a systematic manner, he has deemed it of the highest practical importance to record his observations, for the purpose of emphasizing the value of distinguishing these two forms of intermitting fever. In addition to the hurried narration of illustrative cases, a little time will be taken for the consideration of the mode of recognition of the many sources of origin of paroxysmal fever, and a moment given to the mechanism of fever. It will not be out of place, however, to make a brief reference to the writings of others in this connection, and first to that of the late Dr. Murchison.

In a most instructive clinical lecture,* he called attention to all the forms of paroxysmal fever, giving twelve varieties, viz.: 1. Malarious intermitting fever. 2. Certain cases of typhoid fever. 3. Certain cases of relapsing fever. 4. Pyæmia. 5. Fever from pent-up pus. 6. Fever from ulcerative endocarditis, with or without embolism. 7. Tubercular fever. 8. Fever from lymphadenoma. 9. Syphilitic fever. 10. Urinary intermitting fever. 11. Hepatic intermitting fever. 12. Intermitting fever from morphia.

In addition to examples under each division, he pointed out the clinical features and points of distinction in such detail that it would be supererogatory to enter upon such lines, save in the broadest manner, in this paper.

In the following pages, therefore, cases illustrating the second, fifth, sixth, seventh, and eleventh classes, respectively, of the above, will be recorded, and some new classes will be added, embracing cases of paroxysmal fever due to gastro-duodenal and pulmonary catarrh, to pent-up serum, to forming pus in a confined space.

* The causes of intermitting or paroxysmal pyrexia, and on the differential characters of its several varieties. *Lancet*, May 3, 1879.

Since this paper has been in preparation, a volume of the latest St. Thomas Hospital Reports (vol. xii, '81) came into the writer's hands. Of the many able articles contained therein, there is one by Dr. Ord entitled, "On some cases of Pyrexiae simulans ague." He records a case of ulcerative endocarditis, and one of jaundice with obstruction attended by intermitting fever. Similar cases are detailed below, and hence it will not be necessary to more than refer to them. Cases III and IV of his list are very interesting, and worth repeating in abstract.

CASE III.—Female, æt. 58. Most of life in Mauritius. After returning to England suffered from what was called AGUE—shiverings, heats and sweatings at irregular intervals. At first no pain, but finally increasingly severe pain, attended with vomiting, was felt in the left iliac region. The symptoms repeatedly recurred for months and were regarded as outbreaks of latent ague acquired abroad. Treatment by quinia and arsenic. She finally, after a severe paroxysm, passed a stone the size of a bean from her bladder. Instant relief followed and six months passed away (to time of writing) without any return of fever or sweating.

Case IV is more remarkable, and for the possibility of its like appearing to us, it should be kept in mind.

CASE IV.—A man, æt. 30, never in the tropics, had daily attacks of high temperature, with shivering and sweating. He was sallow, worn and emaciated. His liver was enlarged; his spleen not. He had syphilis. The fever would be reduced by quinia, but only for a time. Thirty grains of iodide of potassium daily cured him, the intermitting fever having been considered by Jenner, in consultation, a manifestation of syphilis.

I. The temperature curve of typhoid fever simulates intermitting fever almost always at some period of its course. During the first week of the disease it is a difficult matter to decide whether a true intermitting is present or not, while in the decline of the disease a distinctly intermitting type is generally recognized. During the period of convalescence one must be watchful that the transient fever which so frequently develops, may not be considered malarial. The temperature during the course of typhoid fever, and the convalescence from it, is, as Dr. Cayley puts it, *labile*. It rises and falls with only the slightest provocation, and frequently takes on an intermitting type.

The following is a rare case of typhoid fever, in which the temperature at the height of the disease was distinctly intermitting. Dying the sixth

day of observation, it was noted that four days before death the patient had daily a congestive chill, followed by a very high temperature. The temperature on the morning of the first chill was $101\frac{1}{2}^{\circ}$ (Fahr.), the evening $104\frac{1}{2}^{\circ}$. The morning temperatures thereafter were on the second, third, and fourth days, respectively, $96\frac{1}{2}^{\circ}$, $99\frac{1}{2}^{\circ}$ and $96\frac{1}{2}^{\circ}$, and on the corresponding evening hour $104\frac{1}{2}^{\circ}$, $105\frac{1}{2}^{\circ}$ and $106\frac{1}{2}^{\circ}$, the latter two hours prior to death. It was considered a case of congestive malarial fever. The autopsy revealed the lesions of typhoid fever about the twelfth day of the disease.

II. It is well known that the fever from pent-up pus is frequently, almost constantly, of an intermitting type. An empyema has frequently been overlooked on this account, but it has never fallen to the writer's lot to have a case that could not easily be recognized. It was different in other cases of deep abscesses, however, and notably in a case—the true nature of which, Murchison says, is almost always overlooked—a case of hepatic abscess.*

The patient, a male, 39 years old, had lived on the Susquehanna, near Harrisburg, and had had chills and fever daily, three weeks prior to admission to the hospital. When admitted he did not seem very sick; he had walked to the hospital, and was permitted to be up each day. He was slightly emaciated and his liver was enlarged. He had daily paroxysms of fever, but the sweating stage continued all night, being more prolonged than in malarial intermittents. He died of hemorrhage from the bowels, one week after admission. The hemorrhage was found to be due to extensive ulceration of the large intestine, not suspected during life, on account of the occurrence of constipation. In addition, at the autopsy a large abscess in the right, and two small ones in the left lobe of the liver were found.

The following table exhibits the temperature record, and shows that we should have considered more seriously the low febrile range:—

		A. M.	P. M.
October	9,	99°	100°
"	10,	99°	101°
"	11,	99°	$102\frac{1}{2}^{\circ}$
"	12,	99°	$101\frac{1}{2}^{\circ}$
"	13,	99°	102°
"	14,	$99\frac{1}{2}^{\circ}$	101°
"	15,	$98\frac{1}{2}^{\circ}$	99°

The history of residence in a malarious locality, the temperature record, the absence of marked local symptoms and of intestinal

* Trans. Path. Soc., vol. viii.

disorders, favored malarial intermitting fever; the absence of enlarged spleen and the low temperature range negated that fever.

A child was seen with a history of daily febrile paroxysms, suspected to be malarial. The child had a severe paroxysmal cough, however, and was losing flesh and strength rapidly. An examination revealed the physical signs of circumscribed pulmonary consolidation, and the mother related the swallowing of a tack some time previous. Ten days afterwards, after a paroxysm of coughing, the tack and a large amount of pus were expectorated. The hectic soon lessened, the resulting cavity rapidly closed and the patient's health was restored. Another example of deep-seated abscess.

Abscesses developing near mucous surfaces are oftentimes very puzzling, at least in their early period.

An abscess of the prostate gland, in a man 48 years old, was one of the most difficult to discern. The patient had been sick a week, and when seen by the writer was in the midst of a febrile paroxysm. He had marked gastro-intestinal derangement, with dry, brown tongue, extreme malaise, daily febrile paroxysms, preceded by chilliness, and followed by profuse sweats, which continued in the night; in addition a dulness of intellect was observed. Six days after the first visit urinary tenesmus was noticed, subsequently rectal distress; an examination revealed a distinct prostatic abscess. It is of interest to note that fever did not occur after the abscess had fluctuated and hence that the forming stage of an abscess sometimes is attended with paroxysmal fever. The following exhibits the evening rise and morning fall, taken on different days:—

13, 4 P. M.,	.	.	102 $\frac{3}{4}$ °,
14, 4 P. M.,	.	.	99 $\frac{1}{2}$ ° ,cinch. anticipated.
15, 12 M.,	.	.	102° , cinch. in lessened doses.
16, 12 M.,	.	.	98 $\frac{1}{2}$ ° , cinch. in increased doses.
17, 11 A. M.,	.	.	98° , cinch. in increased doses.
18, 5 P. M.,	.	.	103° , cinch. in again lessened doses.
19, 9 A. M.,	.	.	98 $\frac{1}{2}$ ° , . 5 P. M., 103°.

A febrile paroxysm was not detected after the 20th, and the table shows that cinchona merely prevented the paroxysms, but did not control them. The case was certainly difficult to analyze. The absence of enlarged spleen, the return of the fever after discontinuing cinchona, and the exhaustive sweats, repulsed the idea of malaria. The appearance of the tongue, the malaise, the headache, and the dulness of mind, with the fever range, made one consider typhoid rather seriously. On the sixth day (19th) after my first visit the local symptoms defined the lesion.

The febrile action then ceased, but the local inflammatory condition continued. It would probably explain the cessation of fever with complete suppuration to say that the soft tumor was not so much an irritant as the hard mass prior to pus formation.

Not only must pent-up or forming pus be considered factors in the causation of a periodical fever, but confined serum or forming serous exudation may undoubtedly give rise to intermitting fever. A case of subacute pleurisy with effusion, in which there occurred in the course of the disease distinct intermitting fever, came under the writer's notice. The usual evening exacerbations were present, but in the morning the temperature had fallen to, or almost to, normal. So marked were the paroxysms that an empyema was suspected, and doubt only removed by paracentesis proving the effusion to be serous. Two similar cases have come to his notice in private practice, both in children. The one, a lad 11 years old, had a dry cough for three weeks, with afternoon malaise and fever. The attendant ordered quinine with but little benefit. An examination of the lungs revealed a large collection of fluid in the left pleural sac, which rapidly disappeared under treatment. The temperature was recorded but once daily for obvious reasons, but at times in the mornings, again in the evening. Invariably an evening rise, a morning fall, were noted; but it never ranged higher than 102° , and there were no profuse sweats following. From the rapid disappearance of the fluid and the speedy renewal of the lad's health, the effusion was called serous and not purulent.

It may seem very trite to record such simple cases, but when, only lately, a child was seen in consultation, ill from a supposed meningitis, but truly so from an actual serous pleuritic effusion, one should feel that nothing is commonplace, and that it is the little things that need to be constantly dwelt upon. With this remark it may be stated that the fever of pneumonia may be intermitting. Later in the paper cases of catarrhal pneumonia will be referred to, but now the croupous variety is considered. Four cases, all in children, are recorded in the writer's note-book. Two of the cases were in his care from the first; two were attended by other physicians coming to him later.

In the first case he was egregiously deceived. The child, *æt.* 4, for five days was well to all intents and purposes, in the morning, eating and playing about with possibly only a slight cough. In the afternoon the temper-

ature would rise to a great height ($104\frac{2}{3}^{\circ}$), and the child would be sick until midnight. Repeated examinations of the lung could not detect a pneumonia until the fifth day. He was misled by the absence of dulness and of bronchial breathing, and the occurrence of tympany over the affected lung, as has been rarely noted.

Case number two, of the same character, occurred in a girl 7 years old. A chill, followed by high fever, with nausea and vomiting, substernal pain and cough, marked the onset. Seen the third day, her temperature in the evening was $104\frac{1}{3}^{\circ}$, with the above symptoms intensified, and a very rapid pulse (140) and rapid respiration (48). Both the fourth and fifth days the temperature was normal in the morning, high at night. On the fifth day bronchial breathing was first noted at the right base; on the seventh day, dulness; on the ninth day resolution began; after the fifth day the fever was continuous. It seemed like a case of retarded pneumonia—as regards physical signs—according to the observation of Dr. Andrew Clark.

Following the outline indicated by Murchison, the next form of intermitting fever he discusses is that due to endocarditis. The following case * of ulcerative endocarditis, the febrile range of which was characterized by daily paroxysms, is of interest. There was no difficulty in recognizing the nature of the affection.

TEMPERATURE RECORD.

	A. M.	P. M.
21,	1—	$108\frac{2}{3}^{\circ}$
22,	$100\frac{1}{2}^{\circ}$	$101\frac{1}{2}^{\circ}$
23,	$98\frac{2}{3}^{\circ}$	99°
24,	98°	$105\frac{2}{3}^{\circ}$
25,	$97\frac{1}{2}^{\circ}$	$103\frac{1}{2}^{\circ}$
26,	99°	103°
27,	99°	$100\frac{2}{3}^{\circ}$

The writer observed it during life, and deems it worthy of being recorded in this connection.

It would be a great surprise to know how many persons, in the latter stages of phthisis, when giving a history of their complaint, say that it was preceded by malaria or malaria broke them down. Over and over again is such a sad tale told us in the medical dispensary, and it is a matter of fact that not only do the laity, but many physicians consider early cases of phthisis as malarial in nature, entirely overlooking the local troubles. When speaking of catarrhal fever, the subject will be adverted to again, but the cases of tubercular origin are sometimes none the less examples

* Trans. College Physicians, Keating.

of intermitting fever, non-malarial. Repeatedly my notes show cases that had been treated for malaria in the early stages. Not only in the formation of tubercle in the lungs, but also in the brain, is the process accompanied by daily paroxysms of fever at times. One case that came under notice was particularly impressive.

The attending physician was going out of town for the summer, and left in the writer's care a little girl 5 years old, in the fourth week of her fatal illness. She had always been a bright child, of nervous temperament and of tubercular diathesis. The illness was of four weeks' duration, marked in the early period by failing in flesh and strength; in the latter period by a chill or chilliness every evening, followed by a night of restlessness and fever. She never complained of headache, nor did she vomit, while her bowels were regular. Eight days before the present attendant saw her, her physician visited her, and attributed the symptoms to malaria; quinine was used. Four days thereafter headache began. The day the writer saw her (fourth week), she had had a slight convulsion and other unmistakable evidences of tubercular meningitis, of which she died in seventy-two hours.

How terrible to be compelled to tell a fond mother the innocent malaria only simulated the baneful meningitis. The writer once made the mistake of attributing a periodical headache to malaria; tubercular meningitis was the cause of the pain. It is seen then, and is well known, that many manifestations of that disease are periodical.

The succeeding case of chronic hepatitis with enlargement illustrates that form of intermitting fever, which is hepatic in origin. The diagnosis was made without difficulty, especially the differentiating from intermittent fever of malarial origin. The following abstract of the history includes all the important points:—

George W., * æt. 43, German farmer, of Manayunk, contracted diarrhœa during the war, which has always shown some tendency to return. Has had malaria; probably has had syphilis; otherwise been very healthy. Family history good. Admitted September 2, 1877, with well-marked jaundice; emaciated, and presented the symptoms of itching, dark colored urine, languor and sleepiness, and a small, slow and feeble pulse.

The jaundice appeared gradually in February of 1877, preceded by several days of diarrhœa. Since then marked dyspeptic symptoms, relieved by attacks of diarrhœa; stools at times clay-colored, at times normal. Some œdema of feet, but ascites never detected. Oct. 2, liver from fourth interspace to two inches below margin on deep percussion, margin smooth

* Trans. Path. Soc., 1878.

and resisting; no pain or tenderness. Oct. 15 to 25, uncontrollable hic-cough. Extreme exhaustion, rapid emaciation, deepening jaundice, semi-typhoid state; death, Nov. 4. Autopsy revealed the diagnosis to be correct.

The temperature record is noted with the remarks of Dr. Guiteras, whose resident physician the writer was at the time, on its curious range, in order to associate the case with a paper on fevers.

					Morning.	Evening.
October	21,	.	.	.	100°	98°
"	22,	.	.	.	98°	103°
"	23,	.	.	.	95 $\frac{2}{5}$ °	100°
"	24,	.	.	.	101 $\frac{2}{5}$ °	96°
"	25,	.	.	.	93°	101°
"	26,	.	.	.	95°	94 $\frac{3}{5}$ °
"	27,	.	.	.	103°	98°
"	28,	.	.	.	95 $\frac{1}{5}$ °	100°
"	29,	.	.	.	97°	98 $\frac{1}{5}$ °
"	30,	.	.	.	97°	98°
"	31,	.	.	.	94 $\frac{2}{5}$ °	96°
November	1,	.	.	.	99°	93°
"	2,	.	.	.	95°	96 $\frac{2}{5}$ °
"	3,	.	.	.	91 $\frac{2}{5}$ °	91°

"I find that every third temperature is pretty regularly a high one, the fall being very great in the two intervening temperatures; so that the rise and fall do not present the usual relations to the morning and evening hours. The curious range of temperature may be due to an intermittent absorption of effete products from the liver, or an intermittent arrest of the oxygenating processes going on in the liver, an arrest that must influence the general temperature, if we remember that in health the temperature of the organ reaches 106°."

In another paper* of the writer may be found reported a case of primary cancer of the gall-bladder.

Early, in fact almost until death, the attending fever was thought to be of malarial origin. The writer, as well as others, made the mistake. Until a few months before her death the fever was distinctly intermitting, with chills; later it became remitting and then continuous. Although there were jaundice and occasional attacks of vomiting, there were no special evidences of localized disease. The spleen was enlarged, and so it was thought to be a miasmatic fever. The change in type, the extreme exhaustion and the emaciation caused this idea to be abandoned. Until death it was obscure. A sufficient cause for the temperature range was found at the autopsy in a suppurative inflammation of the bile ducts, and the healthy portion of a gall-bladder, the remainder of which was the seat of carcinoma. One can see now that more stress should have been laid on the occasional

* Path. Soc. Trans., Phila., '81.

vomiting, the slight hepatic tenderness, the previous history of biliary colic, the persistent and deepening jaundice, and the great emaciation, and thereby a diagnosis been made between miasmatic fever and suppurative fever.

Here will briefly be recorded two cases illustrative of the fever of hepatic origin, not because of one difficulty in their recognition, but because one of them, the first, had been treated for malaria.

This one was the case of M. Mc., æt. 50, who suffered at irregular intervals, often repeatedly in a week, with attacks of severe pain in the epigastrium accompanied by a chill and followed immediately by fever and sweat, and in a few days by jaundice. He died several months afterwards in the writer's care of obstructive jaundice from impacted calculus, after two of these attacks in succession.

When these attacks occurred, every day or every second day, it can be readily seen how a mistake in diagnosis could have been made. Attention to details, however, with the therapeutic test would have been good aid. The paroxysms, by the way, were no doubt due to the irritation of the discharging calculus. The other case was that of an impacted, possibly ulcerating biliary calculus. The history of the case, the jaundice and the local inflammatory changes prevented one from erring.

In addition to the preceding examples of paroxysmal fever, a series of cases will be adverted to which Murchison has not referred to in his lectures, and with the nature of which it is of the utmost importance to be perfectly familiar. Reference is made to catarrhal inflammations of pulmonary, the gastro-intestinal, and the genito-urinary mucous membranes, with secondary intermitting fever resulting therefrom. Especially important is it, for unless the fever is traced to its source, grave organic mischief will become so pronounced as to lead to disastrous consequences. Witness a phthisis following an overlooked bronchial catarrh.

It savors much of the teachings of Broussais, to say that catarrhs are the source of fevers, but there is no doubt that just so far as the philosophic Frenchman erred in that extreme, so do we at the present day err in the other, by attributing most fevers to a zymotic process. Prof. Pepper,* in a timely and instructive address, calls attention to these dangers: That fever is too often considered as due to a zymosis; that zymotic diseases are

* On some of the relations of catarrhal affections. Trans. Am. Med. Assoc., 1881.

of self-limited duration; hence that active treatment is of no avail and especially that the accompanying catarrhs are neglected. Further, on account of these beliefs, the catarrhal process that is often the cause of a fever is overlooked, and thus the commencement of serious local disease is not thwarted.

Reference was made, in another portion of the paper, to the frequency of assuming early tubercular disease of the lung accompanied by intermitting fever, to be due to a miasmatic fever. The following notes illustrate the clinical course of some cases of catarrhal disease of the air passages, which often are the forerunner of so-called catarrhal phthisis. Other examples have been noted, in which there has been only slight catarrhs, without hemorrhage, much cough or emaciation, with attendant fever, occurring in paroxysms.

One of the most typical cases of paroxysmal catarrhal fever came under observation in August, 1880, and was the first to lead to the investigation of this question.

A man, 40 years old, of previous good health and habits, of good family history, and residing in a healthy neighborhood, sought advice for "chills." Daily at 11 A. M. he would have a chill, followed by fever and sweat. The entire paroxysm continued until 6 P. M. His digestion was impaired, and his bowels were constipated. The usual treatment was employed. He reported twice that the chills had ceased to return at once when the medicine was finished. He also reported that his sweats continued throughout the night, and that he was losing flesh and strength. At the third visit he was much dissatisfied, for a former slight cough had grown more pronounced, he had bloody mucous expectoration, and the chills continued. Upon careful examination a distinct area of consolidation at the root of the right lung with attending blowing breathing, and some sub-crepitant rales were found. Active treatment was determined upon, and in six weeks the patient was cured. He has followed his occupation ever since (engineer), is heavier than he ever was, and in perfect health.

Further: A young miss of 20 years, the past winter, was conducted through an attack similar in many respects. Originating in a severe cold, with harassing cough, chest pain, no expectoration and with loss of appetite, nausea and constipation; she lost flesh, and had, the first two weeks of her illness, daily morning chilliness, fever in the afternoon (102°), followed by an exhaustive sweat. During this time the physical signs of a bronchitis were present, with marked localization of the inflammatory process at the right apex. A day of undue exposure and exertion was followed by a severe chill and rapid rise in temperature, with distinct evidence of catarrhal pneumonia at the location indicated above. Chills and fever daily, profuse sweats, emaciation and gastric derangement were prominent for two weeks.

The former symptoms then subsided, but it was fully two months before the lung cleared up, and the patient gained flesh and strength. The family and friends constantly reiterated their opinion that the attack was primarily malarial.

Probably the most difficult, the most occult form of paroxysmal fever of catarrhal origin to recognize, is the one due to that lesion of the intestinal tract. There are no physical signs to betray it, and generally the intestinal derangement is considered secondary to the febrile process. It seems impossible to distinguish the specific from the catarrhal form, save by the presence or absence of the enlarged spleen, the change in the urine of malarial subjects and of the blood when the malaria is chronic, especially when a recent writer tells us that epigastric pain, vomiting and constipation are symptomatic of malaria in children. The following record is a typical illustration of this variety, and is a most instructive and pertinent case:—

'E. M., æt. 5. Inherits a tubercular diathesis from mother. During November and December of 1881 had no appetite, was obstinately constipated, and lost flesh. She became delicate and puny-looking. The latter part of December she was seen on account of the above symptoms and of an irregular fever. The course of the fever was at first difficult to determine, but finally it was found to be distinctly intermittent. She was visited at various hours of the day, and found that at 11 A. M., daily, she would be cold, shivering and begging for extra covering. Her extremities, nose and ears would be very cold, her lips bluish, and the features pinched. At the same time the pulse would be rapid and the temperature in the mouth 102° . In a half hour the exterior warmed, and very soon she would have high fever, the temperature rising to 103° – $103\frac{1}{2}^{\circ}$. The febrile stage lasted three or four hours, and was not followed by profuse perspiration. Save weak and without appetite, by night she would be perfectly well. Quinia was given in continuous doses at first, afterwards in doses to anticipate the paroxysm; but without any good effect. The paroxysms were lessened in severity only while the already poor appetite was made poorer and the digestion more impaired; for two weeks an anti-periodic treatment was continued, and at the same time laxatives were used to overcome the constipation; at this time (January, 1882) she was thin and worn, the paroxysms of fever were daily, the appetite was very poor, the breath offensive, the tongue covered on the dorsum with a yellow-white fur, pointed, and with no papillæ; vomiting occasionally occurred, and always some pain after eating; the bowels remained obstinately constipated. It seemed to me, after a time, the fever was a secondary matter, that the gastro-intestinal disorder was primary, and that such disorder was subordinate to the diathetic constitution. Hence she was placed on small doses ($\frac{1}{2}$ gr.) calomel

with bicarb. of soda (5 gra.) every three hours. In three days cod liver oil with syr. of the hypophosphite of lime was added to the treatment. At once she began to improve; her appetite first, then her bowels became more regular. In two weeks the child rapidly improved under this treatment, after being treated previously for more than two weeks for malaria. It may be added here that twice or three times E. became constipated with similar febrile symptoms noted above, and that the parents, without my advice, cured her with the cod liver oil mixture.

A case very similar to the above was also seen. It is useless to report the details of the case; remedies directed to the gastro-intestinal catarrh, with accompanying intermitting fever, effected the cure.

A case of stricture of the pylorus, in its course, at one time presented daily chills and fever. Quinia did not control the paroxysms. During the time of the fever, and for a week afterwards, the stools of the patient were composed of mucous or membranous casts of the intestinal canal or of a pultaceous mucoid discharge.

These cases incontestably prove the proposition that intermitting fever is often due to catarrhal inflammation of the intestines, and that remedies directed to this locality alone will cure the disease.

This clinical record will be closed by the report of an observation of a case, the nature of which is somewhat obscure. It is not given, therefore, without some misgiving. It appears that the only title that could be applied to it would be paroxysmal fever of neurotic or hysteroidal origin.

The patient was 25 years old, of a rheumatic diathesis and nervous temperament. She presented a history of "chills and fever," recurring at irregular intervals for two years. The paroxysms were of the quotidian type and the attacks lasted one or two weeks. Considered to be malarious; quinia or cinchona was always given by her attendant, and the usual remedies for malarial toxæmia used, without cutting short or preventing the attacks. The writer attended her through two attacks. They were of the following nature: Preceded by dyspeptic symptoms for a few days, a violent chill attended the onset of the attack, accompanied by severe headache, with tender spots and one or more localized points of pain in the body. In one of the attacks the pain with the first chill was fixed at the end of the spine with exquisite tenderness; in another it was in the epigastrium. The chill was an hour in length and followed by fever. With the fever the face would flush, the eyes "burn," and the skin be hot and dry. The temperature would rise to 103° or more, the pulse be full, bounding, rapid. Evidences of gastric catarrh with constipation were also noted. During the paroxysm the most pronounced emotional disturbances were manifest, so that had fever been absent it would have been without difficulty considered a case of hysteria. A sweating stage of two hours followed the fever.

The paroxysms recurred daily for a week, but with the repetition of each one the pain would be seated in another portion of the body—in the occiput, the shoulder, or the knee-joints—while the emotional disturbances would be also present. The pain was described as unbearable, and could not be influenced by almost incredible doses of the usual anodynes. Quinia was given in enormous doses in the first attack, without any beneficial influence.

The fact that the paroxysms occurred towards night and that they were accompanied by hysterical symptoms of a high degree, the inutility of quinia and the absence of enlarged spleen rendered the opinion that the case was of neurotic origin, probable.

The second attack was very similar. Vomiting was, however, a more persistent symptom. The duration was about one week, and it appeared to yield to remedies addressed to the hysteria and the gastric irritability. The whole tenor of the patient's life has changed since then, so that for two years she has not had a return of the supposed malaria, notwithstanding she is exposed to the same malarious influences.

Time will not permit a review of the various affections in detail, in order to establish a differential diagnosis between these simulative disorders and a true intermittent. Any attempt at a positive diagnosis of paroxysmal fever, however, should not be made without keeping in mind the following proposition: In the first place, one would say that given a case with a chill and fever, a diagnosis of intermittent ought not be made from the nature of the first paroxysm, unless it be vital to do so, as in a pernicious intermittent. Then, if such a case is presented that yields but partially to anti-periodics, they should be discontinued and a fresh start in the diagnostic inquiry taken.

In order to fully establish a diagnosis a careful study of the antecedents of the patient should be made relative to previous health, habits, place of residence, and family history. Then, in favor of malarious intermittent, we should, after this study, expect a morning hour for the chill (Flint), the well-known changes in the composition of the urine, and if chronic, the enlarged spleen and the pigment granules in the blood. If with one or more of these favorable factors present we could exclude all possible source of organic disease, by an examination of each individual organ, the blood (leukæmia), the eye ground (tuberculosis), the lungs, liver and gastro-intestinal tract, we would be warranted in the diagnosis of malarial intermittent.

It seems, further, to be of value to note that emaciation of a high degree is more common in non-malarious intermittents.

The same may be said of exhaustion. The latter occurs to a certain degree, and is attended with a pronounced anæmia, so easily recognized as of malarial origin. Then, too, a long sweating stage and a low febrile range rather disprove the presence of the malarious influence.

Enlargement of the spleen is not to be considered, in acute intermittents, as of little moment. In a series of twelve cases of intermittent in children, eight presented the enlargement, which had subsided a year after the first examination.

There is but little doubt that fever is of neurotic origin, and the examples which have been recorded to-night more aptly illustrate this cause than any other class of cases. The profession is so thoroughly imbued, however, with the idea of no fever, unless a zymosis or blood-poisoning, that it is of practical value to refer to the mechanism of fever briefly. As shown by others, disastrous results oftentimes ensue by addressing means to the cure of a zymosis, or by passively allowing a febrile process to continue its supposed self-limited course, when actually a zymosis was not present, and remedies otherwise applied would have been beneficial. The reference to the mechanism, therefore, is to show that often fever is of a reflex origin due to peripheral irritation—a neurosis.

The element of intermittency itself is a powerful argument in favor of its neurosial origin. This is not the time to engage in philosophical speculation, or to demonstrate the relation of the fundamental principle of the rhythm of motion so grandly elaborated by Spencer; suffice it to say that to no other set of tissues or systems could we look to but the nervous system for an explanation of intermittency. Aside from this, however, in the masterly study in morbid and normal physiology by Wood, on the mechanism of fever, we find sufficient argument and proof "that a depressing poison or a depressing peripheral irritation acting upon the nervous system which regulates the production and dissipation of animal heat," causes fever.

Among the illustrations presented to-night, there are some which strongly indicate the reflex origin of fever from peripheral irritation; witness the case of vesical calculus or of gall-stone. By what other supposition could the phenomena be explained? Likewise, though with an element of doubt intermingled, in the cases of gastro-intestinal catarrh, the fever may be considered as

due to reflex processes. In the other cases the fever is, no doubt, due to the absorption of a poison which acts upon the nervous system, and as opposed to Charcot and Billroth, one would think that the phenomena of intermittency is due not to paroxysmal discharges of pus or poison into the blood, but to rhythmical responses of the nervous system to a constantly-acting poisoned blood.

3706 POWELTON AVENUE.

SUPPLEMENTARY REPORT OF REMARKS IN DISCUSSION OF
DR. FORMAD'S PAPER.

DR. SHAKESPEARE regretted his inability to be present to open the discussion in accordance with the request of the President, and thanked the Society for this opportunity of expressing his views. He had been much interested by the opinions and by the review of the status of the tuberculosis question presented by the author. There were, however, very many points assumed as demonstrated, and positive statements advanced in the elaboration of Dr. Formad's paper, which Dr. Shakespeare believed to be without sufficient foundation. But he would not, at this time, enter into a general criticism. He preferred to await the detailed observations which the author promised shall be forthcoming in support of the many statements and conclusions he has thought proper to announce in advance. He intended to limit his remarks to-night to some differences between himself and the author as to statements made by the latter concerning a recent visit to Koch's laboratory. Dr. Shakespeare also had been in Berlin last summer, and had then enjoyed the privilege for about a month of working under Koch and his assistants during six or seven hours daily.

1st. The author had declared, in terms far less equivocal than those printed, that Koch's policy is to hinder or prevent strangers who visit the Gesundheitsamt from retracing his now famous experiments upon tuberculosis, and stated that no one had ever been permitted to inquire into the infectiveness or parasitic nature of tuberculosis, save one man,

2d. The author had further announced that Koch has so far modified his views that he now admits that neither the form, size and aspect of the tubercle bacillus, nor its want of individual motion, nor its peculiar behavior towards staining fluids, distinguish it from many other bacilli.

Dr. Shakespeare regarded these statements as misrepresentations of Koch's animus, as well as of his present opinions. He felt impelled to thus publicly express himself, because perhaps every member present had known of his late visit to the Kaiserlichen Gesundheitsamt. To be silent under these circumstances would constitute a tacit assent to these declarations—a false position in which he was unwilling to be placed. Moreover, the grave importance of this whole question; the presumed desire of this learned Society to be possessed of all the evidence bearing upon every phase of it; justice to the fairness, honesty and consistency of the distinguished author of the bacillus theory of tuberculosis, whether it be true or false,

forced him to express now his dissent from the foregoing declarations of his friend.

Previous to the announcement of the discovery of the "tubercle bacillus," he had been most favorably impressed by the exactness and completeness of Koch's labors in the final establishment of the parasitic nature of anthrax (French, charbon; German, milzbrand; English, splenic fever), as also by the evident caution and reliability of that investigator. This had prepared him to begin the examination of the grounds of Koch's startling claims regarding the nature of tuberculosis with no small degree of respect for their author. At that time he had no definite views concerning the cause, infectiousness, or contagiousness of tuberculosis. Certainly he did not commence this examination with a mind wholly preoccupied by a theory of his own which he thought to be in conflict with that of Koch.

He had not gone to Berlin for the purpose of discovering there the truth or falsity of the claims for the "tubercle bacillus." On the contrary, recognizing the growing importance of research among the various forms of bacteria as possible causes or modifiers of pathological processes, and having personally experienced much trouble in prosecuting such studies whilst following described methods, and, through his intimate relations with the University of Pennsylvania, having known of similar difficulties in the Pathological Laboratory of that school of medicine, he had at length determined to obtain, if possible, ocular demonstration of Koch's classic methods of isolation, culture and study of minute organisms, and had become one of "the pilgrims" to that Mecca toward which Dr. Formad himself had directed his steps only a few weeks before.

Arrived in Berlin he had been most cordially welcomed at the Gesundheitsamt by Dr. Koch and his corps of accomplished collaborators, and every possible facility for furthering the object of his visit was most willingly and courteously tendered during the whole of his stay, though doubtless at the cost of much inconvenience, for, beside work upon important investigations, active preparations for the departure of the cholera expedition to Egypt were then in progress. He could say that he had never spent a month with more pleasure or profit. While it had not been his desire to give especial attention to the "bacillus tuberculosis," more than to the bacillus anthracis and to other bacteria, yet as far as his wish extended, and the limited time at his disposal served, in his practical work the "bacillus tuberculosis" was not neglected.

He felt impelled to say, in the most emphatic and unmistakable language which he could use, that he himself was not only readily permitted to go as far as he wished in the investigation of the tubercle bacillus, but furthermore, on no single occasion did he meet with any hindrance whatever, or perceive the slightest indication of a desire on the part of Koch to prevent the retracing of his experiments upon that subject. He had heard of no one having met such a difficulty there other than Dr. Formad. The only person who, previous to the presentation of the paper under discussion, had to his knowledge published an account of personal work done upon tuberculosis in Koch's laboratory was Watson Cheyne, England, whose report

amply testifies to Koch's willingness to have his experiments examined. Dr. Formad, in his communication as printed, excepted this work of Watson Cheyne, perhaps wisely, for he several times quoted for other purposes this same report.

If Dr. Formad, during the three or four days of his attendance at Koch's laboratory, did not experience an enthusiastic reception, and, as he intimated, was not permitted to experiment upon the pathogenic qualities of the tubercle bacillus, he might far more reasonably have attributed this coldness to an irritation naturally produced by his published remarks in which Koch had been accused of unscientific work, and the insinuation been offered that the researches made at the Imperial Health Office had been unduly influenced by Kaiser Wilhelm, than to have assumed from his reception that Koch habitually objected to have any one look into the genuineness and reliability of his work upon tuberculosis. Indeed, the simple fact of his admission at all under the circumstances, could fairly have been regarded as evidence of Koch's willingness to open his laboratory even to an opponent whom he regarded as unfair. The Gesundheitsamt is a department of the German Government. Koch and his chief assistants are officers of the German Army or Navy. They are all intensely loyal to their Emperor. They believed that Dr. Formad had purposely and unjustifiably stepped outside the proper sphere of a purely scientific communication to publish a reflection insulting to them and their Kaiser.

Before dismissing this indirect attack upon the reliability of Koch's published observations upon tuberculosis, Dr. Shakespeare took this opportunity to say that his personal observation of Koch, as well as a careful examination of his publications, had led him to the conviction that the whole medical fraternity does not possess a more painstaking, capable, cautious, thoroughly honest and reliable investigator of the causes of disease than the distinguished discoverer of the tubercle bacillus. He would speak in similar terms of those of the corps of official co-laborers at the Gesundheitsamt, with whom he had come in contact sufficiently often to form an opinion.

The second statement above mentioned, namely, that Koch has now essentially modified his views concerning the characteristics of the tubercle bacillus, was next examined. Dr. Shakespeare could only say that Dr. Formad's extraordinary announcement was the first and the only information upon this point which he had received. Certainly he had heard nothing and seen nothing whilst at the Gesundheitsamt, which could in any manner confirm such a statement. It is true that, while at Berlin, the author had related to him his interview with Koch, and had said that the latter had been far less dogmatic than he had expected, mentioning among other things a little friendly controversy concerning their opposite views in which Koch had seemed quite willing to admit the *possibility* that under favorable circumstances, the tubercle bacillus might develop a flagellum at its extremity and thus become endowed with individual motion (Dr. Formad had claimed to have seen this motion), and had appeared quite willing to admit also the *possibility* that in the course of time it might be discovered that other bac-

teria would react toward staining fluids in a manner identical to the reaction of the tubercle bacillus. But an admission that certain things *may be possible* and a statement, based upon present knowledge and experience, that they do exist, or are even probable, are quite different matters. During Dr. Shakespeare's work upon the tubercle bacillus in Koch's laboratory, which was after the termination of the short visit of Dr. Formad, he was taught to differentiate the tubercle bacillus from all other bacilli by means of its characteristic reaction, now well known, toward certain staining agents, no less than by its peculiar size and shape, as seen under high magnifying powers (Zeiss' $\frac{1}{2}$ was generally used for this purpose). The statement that the author of the bacillus theory of tuberculosis has practically withdrawn his claim that there is something characteristic in the staining of the tubercle bacillus and in its morphology which distinguishes it from other bacilli is the more astonishing and incredible because of the fact that, besides the existence of overwhelming testimony from all quarters of the globe in confirmation of this original claim, even Dr. Formad, however persistently in print he may assail this claim of peculiarity, is himself in the habit of differentiating this minute organism from all other known bacilli for purposes of diagnosis and of demonstration to his pupils *by means of this self-same characteristic coloring and morphology*.

Although it had not originally been his intention to discuss them this evening, Dr. Shakespeare briefly considered Dr. Formad's claims of discovery of the etiology of tuberculosis as set forth in his two papers. This author had been among the first to controvert Koch's theory of tuberculosis. Somewhat more than a year ago he made the first announcement of his views. In this communication the author advanced a theory of his own, which he believed to be opposed to that of Koch. He claimed that there is no necessity for the action of a specific agent in the production of tuberculosis, and that therefore such a specific agent can have no rational existence. This claim was, in the main, based upon his belief in the discovery of an anatomical peculiarity of those animals known to be especially prone to tuberculosis. This peculiarity he thought to consist essentially in a narrowing of the connective tissue lymph-spaces in certain animals—the scrofulous—and to be either hereditary or acquired. He claimed that the inflammatory process in such animals, whatever be the exciting cause, is necessarily tuberculous.

On the occasion of the presentation of his first paper, Dr. Formad undertook to demonstrate this reputed anatomical peculiarity by the exhibition, under the microscope, of a number of anatomical preparations. At that time Dr. Shakespeare had regarded that demonstration as far from satisfactory or conclusive. In the first place, no single section showed lymph-spaces. In the second place, the method of preparation followed (that for ordinary histological examination—hardening in alcohol, cutting thin sections, staining these with carmine, mounting them for examination in Canada balsam) naturally was not capable of demonstrating lymph-spaces; not one silver or gold preparation was exhibited. Indeed, this common and satisfactory method of studying lymph-spaces had apparently not even been

resorted to, for it is to be presumed that the most positive and demonstrative specimens in the possession of the author were those selected for exhibition. It is true that some of the sections under the microscope showed a cellular hyperplasia of the connective tissue—an appearance by no means new to the scientific world. And this was the sole evidence presented in support of a reputed discovery concerning an important anatomical peculiarity of the lymph-spaces of so-called scrofulous animals, upon which an exclusive theory of the etiology of tuberculosis has been erected by the author and claimed to be demonstrated.

Recognizing the importance of that reputed discovery, this learned Society had at once appointed a committee, consisting of its most experienced microscopists, to examine anatomical preparations which Dr. Formad should lay before it in proof of his announced discovery. Nearly eighteen months have since elapsed, and yet, during all that time, not one preparation has been submitted for examination by that committee.

In the paper at present under discussion, the author complaisantly refers, for proof of his so-called discovery, to the evidence brought forward in his first paper, and supplements this by *promising* with apparent self-satisfaction the future publication of corroborative observations by some independent investigators. Other criticisms might justly be urged, but in view of the foregoing facts alone Dr. Shakespeare believed himself sufficiently warranted in contending that the basis of Dr. Formad's opinion concerning the etiology of tuberculosis has not been established, and also in suggesting that instead of that opinion being referred to as a "theory" against the theory of Koch, it was scarcely yet entitled to be dignified by the name of *hypothesis*.

Furthermore, even admitting that this *hypothesis* concerning the anatomy of the lymph-spaces of the so-called scrofulous animals were, by the most indisputable evidence, demonstrated beyond the possibility of doubt, it still contains absolutely nothing which by itself either necessarily supports the conclusion of Dr. Formad regarding the non-specificity and non-infectiousness of tuberculosis, or antagonizes the claim of Koch for the specific pathogenic qualities of his tubercle bacillus. When, if ever, this hypothesis shall become a fixed and determined fact, we shall then be placed only one step nearer a correct understanding of the etiology of tuberculosis. The reason of that peculiar *predisposition* which certain animals are known to show towards tuberculosis may then have been satisfactorily explained. But what the *exciting cause* of that peculiar malady may be, is an entirely different question. Whatever this may be, it can be readily understood that its power of destruction would naturally be favored by such an anatomical peculiarity. Such an "anatomical peculiarity," if it really exist at all, can be easily turned to the support of the bacillus theory. The claim of Koch is not that the tubercle bacillus is endowed with pathogenic qualities which under any and all circumstances are capable of exciting tuberculosis. He himself declares that for the calling forth of these powers a suitable soil and conditions favorable to growth and propagation are essential.

Finally, Dr. Shakespeare thought it proper to define his own position with regard to the etiology of tuberculosis. He wished it to be distinctly understood that it was not from the standpoint of a follower of Koch, who accepted all of that investigator's conclusions, that he had offered the criticisms which he had made. In the consideration of such a grave question as the one then confronting him, he regarded it as obligatory to exact the same degree of rigid proof from friend as from foe, whether advanced on the side of popular opinion or against it. He therefore had not hesitated to express objections to the opinions and statements advanced by his friend.

Dr. Shakespeare admitted, as absolutely established, the power of the tubercle bacillus, under favorable conditions, to produce a genuine and virulent form of tuberculosis. He did not admit that it has been positively demonstrated that no other agent may also be capable of producing the disease; on the other hand, he denied that it has been satisfactorily proved that any other agent is capable of exciting tuberculosis. He believed the proof strong that under certain favorable conditions, tuberculosis is an infectious disease, and that, at least frequently, the infecting agent is the tubercle bacillus. He saw no valid reason to deny that, under certain favorable conditions, tuberculosis may be conveyed from person to person, and in this sense be termed a contagious disease. Whether or not the tubercle bacillus be regarded as the only agent capable of exciting tuberculosis, its virulence is certainly incomparably greater than that of any other known agent. He therefore failed to appreciate the wisdom or the logic of those who, admitting the virulent qualities and propagative power of the tubercle bacillus, yet, because of a lingering suspicion or even of a decided belief that other agents could produce this terrible disease, would still decline to guard against possible infection or contagion. He regarded the tubercle bacillus, when present, as an infallible sign of the presence and activity of the tuberculous process. On the other hand, its absence, unless after repeated and long-continued searches by competent observers, does not positively warrant a negative conclusion. He therefore saw in the tubercle bacillus, an important means of differential diagnosis in obscure cases. From its reported presence in some cases earlier than the physical signs could possibly determine a diagnosis of phthisis, he was inclined to think that it may become of inestimable value to the skillful practitioner to forewarn him of the beginning of that formidable malady which, if curable at all, must be combatted from the very onset.

DR. WOODBURY said that at least two distinct questions had been submitted for discussion: Is consumption contagious, and Is the bacillus tuberculosis the efficient and only cause of consumption? One of these is not necessarily the complement of the other. Consumption may be contagious without being caused by a bacillus, and bacilli might cause consumption without rendering it contagious. The first question he thought should be decided by clinical experience, the second by clinical experience with the aid of morbid anatomy and mycology. Time would permit only a very brief presentation of the arguments in favor of the views which he held, and he therefore would at once state his conviction, and he believed

the experience of others would agree with his own, that pulmonary consumption as ordinarily met with is not a contagious disease. Since the definition of a disorder must be made from the clinical picture presented by the majority of cases, he would say that the typical case of consumption does not present any evidence of possessing a contagious character. The question as to the communicability of consumption under exceptional circumstances, he regarded as a very different one from the former. Meningitis or nephritis may be communicated under peculiar conditions, but this would not warrant the clinical teacher in describing them as contagious, at least in any ordinary acceptation of the word. He had seen a number of cases of consumption which had occurred in members of one family living under the same conditions, but had never met with a single case where the evidence of contagion was conclusive. Even cases of apparent communication from husband to wife or *vice versa* could be satisfactorily explained to his mind on other grounds than of direct transfer of the disease by organic or organized particles. The susceptibility to phthisis can be native or acquired, it cannot be transmitted by particulate infection.

With regard to the etiology of consumption, it would appear that there are several varieties of the disease which are indistinguishable by ordinary physical signs. In the first place there are two classes of cases which stand apparently identical, differ in the microscopical characters of the sputum; one contains the alleged bacillus tuberculosis, the other not. This leads us to a classification of bacillary and non-bacillary tuberculosis. In the latter class of cases, in addition to syphilitic phthisis, pulmonary actinomycosis, and zoögleic tuberculosis (a form of mycosis recently described by Malassez Vignol*), there are included cases of ordinary pulmonary phthisis but *minus* the bacillus. In the first class, therefore, the question arises, "are the bacilli necessarily the cause of the morbid phenomena?" He thought that they are not essential, (1) because it has been shown that consumption can be due to other causes and can pursue its course without their appearance, and, (2) because they are apparently not a necessary element of tubercle. The bacilli have undoubtedly a certain diagnostic and prognostic value, but their appearance can be accounted for on the hypothesis of their being a mere concomitant of pulmonary consumption, even though it could be shown that they increase its fatality. He was surprised that with such abundant opportunities for observation, clinical teachers had not been able to convince the world or themselves that consumption is contagious, until they are shown something under a microscope. He was more than surprised that Prof. Austin Flint had announced his adherence to the new doctrine, that "pulmonary consumption is due to the bacillus tuberculosis, and arises in no other way."

DR. GEORGE HAMILTON said that after a practice of more than half a century, he had seen no case of pulmonary consumption that could, rationally, be attributed to contagion. In two or three families, where several members were affected with this disease, attempts were made to

* Jour. Am. Med. Assoc., Feb'y 16, from Archives de Physiologie.

refer it to contagion, but without any sufficient proof. It is to be borne in mind that great repugnance sometimes exists in a family to admitting an hereditary tendency to this affection, scrofula, and certain other maladies.

DR. DUNMIRE said that on the question, "Whether or not simple inflammation of serous membranes could lead to tuberculosis in the non-scrofulous," he would say that he had the notes of a case in which the post-mortem proved death to be caused by phthisis pulmonalis, in which the primary trouble seemed to be the fracture of two ribs on the right side.

While both lungs were involved, the pleuritic adhesion of the right side was almost entire.

An intimate acquaintance with the family, both before and since the death of this patient, has failed to show any sign of tubercular trouble, and as far as he knows, none of this connection have died of the disease.

SOME REMARKS UPON THE TYPE OF TYPHOID FEVER PREVALENT LAST WINTER, WITH PATHOLOGICAL SPECIMENS.

Read May 21, 1884.

BY DR. GEORGE W. VOGLER.

Mr. President:

The following brief notes may prove of interest to those present this evening.

I desire more particularly to call attention to the unusual prevalence of typhoid fever in our city during the past winter, to the grave type of a majority of the cases, and to the unusually numerous complications.

These notes refer only to the cases that came under my notice during my service at the German Hospital, for the months of January, February and March of this year. I might state in passing that what is true of these cases, is also true of those occurring in my private practice.

During the three months twenty-four (24) cases were admitted into the wards, more than half the entire number of cases treated in that institution last year. As three (3) of the above number were brought into the house in a moribund condition, dying immediately or shortly after their admission, the remarks to follow will not appertain to them.

The balance, twenty-one (21) in number, receiving treatment at our hands, may be divided into the following two classes:

possessing also the additional advantage of destroying the fœtor of the stools.

In those cases marked by great nervous excitement or actual delirium, and in which opium or bromides seemed of little or no avail, hypodermic injections of ten grains of muriate of quinia acted apparently as a hypnotic. In passing, I would like to state here that, in one instance, the hypodermic use of this remedy produced an abscess that gave us much trouble to heal. Our statistics at the hospital, however, show this to be a rare occurrence, as out of about 250 hypodermic injections of this remedy in various diseases, abscess resulted in but two instances.

An interesting feature noticed was that whenever the catamenia occurred it was invariably attended by an increase of temperature. In one instance (Emory), where a relapse was supposed to have taken place, the temperature ran from normal up to 103°, and gradually subsided with the cessation of the menses. This phenomenon was noticed to occur twice in this particular case.

Another matter of interest was that two of the female employees of the hospital were stricken down with typhoid fever, barely escaping with their lives. The duty of one of these women was to empty and disinfect the receptacles for the excrement, etc., and to care for the soiled linen from the typhoid fever wards. The other was employed to wash this linen. I certainly would not like to draw the moral in the first instance: "Do not use disinfectants."

Of course the treatment of the grave and complicated cases was adapted to suit each individual case. In the milder type of the disease, simple common-sense treatment was used, preferably the mineral acids, 5 or 10 drop doses of nitro-muriatic dil., changing to the dil. sulphuric, if diarrhœa became troublesome. Where quinia was indicated, I gave it in small but repeated doses instead of one or more large doses, rarely giving over seven grains daily. This method has always obtained the most satisfactory results for me.

Dover's powder for sleep, and opium, silver, ergot and resorcin for diarrhœa and hemorrhage, were used when necessary. Also turpentine when indicated.

A favorite method of giving small doses of turpentine, quinia, and dil. nitro-muriatic acid together, in cases where the symptoms existing demanded the need of all these remedies, was to

use a menstruum of Syr. Ext. Glyc. Fld. in the proportion of 2 $\frac{f}{3}$ of Ext. Glyc. Fld. to 1 $\frac{f}{3}$ of Syrup, flavored with Ol. Gaultheriæ. This makes a smooth, pleasant and palatable mixture.

I desire to show the following specimen of perforation of the bowels, the other interesting pathological specimens having been mislaid.

Anna Stötz, æt. 20, domestic, single. Admitted into house January 28, the seventh day of the disease, having had several severe hemorrhages previous to admission. Some pulmonary complications of an acute nature also present; these rapidly disappeared under proper treatment. On the 30th profuse hemorrhages occurred, seven or eight during the twenty-four hours.

Treatment: Gallic acid by bowels; opium, ergot and silver by mouth; strychnia hypodermically. By this treatment hemorrhage was controlled and fair hopes of recovery were held out.

About the 5th of February hemorrhage again suddenly came on, which also gave way kindly to treatment. On the morning of the 10th, sudden collapse with intense pain in abdomen, followed rapidly by death.

The post-mortem examination revealed very extensive ulceration and a perforation situated close to the inner border of an ulcer some two inches in diameter, situated within a short distance of the ileo-cæcal valve.

WARNING TO THE MEDICAL PRACTITIONERS IN REGARD TO THE USE OF JEQUIRITY.

BY M. LANDESBURG, M. D.

Read May 21, 1884.

THE medical journals have not failed to inform their readers that a new remedy has been introduced by Wecker into the oculistic therapeutics, which, by its prompt, energetic and sure action in trachoma and pannus, by far surpasses all the other methods of treatment ordinarily used in this affection. These glad tidings have been corroborated by the casual publication of notes and comments on the many excellent results obtained by Wecker and his followers by the new procedure, which consists of applying an infusion of jequirity, of a given strength, to the surface of the palpebral conjunctiva, in order to produce rapid suppuration, and by means of the latter to promote absorption of the trachomatous infiltration. The method of treatment is based upon the same idea which led to inoculation of blennorrhœic pus in cases of pannus. I have not seen any medical paper at my command dwelling upon the great dangers in which the diseased

eye is apt to be involved by the process of suppuration, and by the possible excess of reaction. I have not seen pointed out the fatal consequences which may develop in some instances. The glittering side of the question has only been made conspicuous, and it has not been considered worth while to show also the reverse of the medal. That this sin of omission may prove a source of trouble and mischief to some medical practitioners is obvious. The general physician is not in the position to follow all the intricacies of the experiments with the new drug. He relies for information on his medical journal, and the latter tells him of jequirity as the panacea in trachoma and pannus. Now suppose he just has such a case under hand, which had proved rebellious to the treatment with the usual remedies. He finds jequirity highly spoken of in his journal, and he avails himself of the opportunity to win by easy means the battle and the honors connected with it. And now it happens that he makes matters worse, that the very existence of the organ is endangered by the new treatment, the full recovery of which he has expected with such confidence! But I do not draw the picture from imagination; I do not speak of possibilities, but of facts that have already occurred. I relate incidents of practical life which were communicated to me by general practitioners from different parts of our country. There was excess of reaction in some instances, and implication of the cornea in others. Besides, I have received letters in which physicians appealed to me for information considering the action of jequirity and the expediency of using it in trachoma and pannus, urging me at the same time to lay before the profession the results of my experiments in the matter.

In answer to the many queries, by which I feel deeply honored, I have only to state briefly as follows:—

I have not had any cause to abandon my usual method of treatment in instances of trachoma and pannus, which has still given me the most satisfactory results even in the most obstinate and inveterate cases. My interest in jequirity has thus far been merely theoretical, and the successes reported from one side, and the failures brought forward by the other, have only served to uphold my position of objective observation. The question is by no means ripe for verdict. Only the future can show whether jequirity will gain a permanent place in the oculistic therapeutics, or will share the fate of the many other "new remedies," to

sink into well-merited oblivion after a short period of dubious fame.

But if I cannot produce the results of my own trials with the use of jequirity in trachoma and pannus, I am able to give my experiences on the action of jequirity, which I have gained from experiments made by others. The issue of these experiments in nine cases, which came under my observation, indicates plainly enough the course the general practitioner has to take in regard to jequirity.

Of the nine cases above mentioned, the result of the treatment with jequirity was negative in five cases of trachoma and pannus. There was no improvement whatever, but no injury done either. Two cases presented with trachoma and pannus, deep corneal ulcerations, which were asserted to have developed during the treatment with jequirity. Before the latter had been started, patient had enjoyed fair vision. One patient, with xerophthalmus of both eyes, suffered the loss of the right eye in consequence of panophthalmitis, which had set in on the fourth day after the application of jequirity. There was not the slightest improvement in the left eye. A girl who had done nothing for her eyes up to the time of the treatment with jequirity, presented herself six weeks later with the following conditions: Lids thickened; palpebral conjunctiva intensely swollen, covered thickly with large granulations, and furrowed with tendinous cicatrices. Both corneæ opaque and vascular.

REPORT OF THE OBSTETRIC DEPARTMENT OF THE
PHILADELPHIA HOSPITAL FOR THE QUARTER
ENDING APRIL 30, 1884.

Read May 23, 1884.

BY THEOPHILUS PARVIN, M. D.

Professor of Obstetrics and Diseases of Women and Children in Jefferson Medical College.

BY the kindness of Dr. Bernardy, my associate in term of service at the Philadelphia Hospital, the entire charge of the obstetric department was given me, while he had that of diseases of women and children. It seemed to me that by this division of labor both the interests of patients and of medicine would be best subserved; and I desire publicly, as I have done privately, to express my gratitude to Dr. Bernardy for his consent

to this arrangement. Further, let me gratefully acknowledge the zealous and faithful work of the *internes* serving under me, in the collection of statistics, and making observations, without which the preparation of this paper would have been impossible. My debt to these gentlemen, Doctors Phillips, Parkhill, Randall, Lazarus, and Voorhees, is very great. Some of the statistics and observations, or their results, have been given elsewhere; others will be presented you now, and still others wait another opportunity.

And now, gentlemen of the Philadelphia County Medical Society, unexpectedly invited to read a paper before you, and thanking you for the honor, my endeavor will be to present facts rather than theories, results more than reasoning, hoping that possibly some of the facts and results may be of present interest and of future use, and knowing that the discussion they may evoke will have these characteristics.

During my term of service at the Hospital seventy-two women were confined; this number, however, includes two cases of premature labor, and one of miscarriage at six months and a half; there was one case of twins. In sixty-nine cases the vertex presented; presentation of a foot, of the breech, and of the shoulder, each occurred once; the presentation in the case of miscarriage is not given. Forty of the seventy-two mothers were primiparæ. Of seventy-three children born, thirty-nine were females, and thirty-four males—a preponderance of female births which is at least remarkable.* Fifty-one of the mothers were white, twenty-one black. The average weight of the white children was seven pounds and a little more than two ounces; that of the colored children, seven pounds thirteen ounces and a fifth: there was thus a difference of eleven ounces in favor of the latter. The heaviest child was a white one, its mother a primipara; its weight

* The general relation between female and male births is 100 to 106. Illegitimacy slightly lessens this proportion, that is increases the number of females born; and this is a factor adding to the number of female births at the Philadelphia Hospital, for illegitimate births are there the more numerous, but still it is not sufficiently potent to entirely reverse the law. This abnormal disparity between male and female births is not a mere accident of the three months, for taking all the births of 1882 and 1883, and adding those of the first quarter of 1884, I find the number is 371, and of these 173 were males, and 198 females. It would be interesting to examine the hospital record for a long series of years, and ascertain if this disparity is the same: and this it is my intention to do.

While referring to the normal relation between male and female births, and the effect of illegitimacy upon it, I may mention the curious contradictions of these laws given by the statistics of Roumania: These show that the proportion of female to male births is 100 to 116, and, further, this proportion is not changed by illegitimacy.

was nine pounds and twelve ounces. Comparing the difference between white male and female children, it was a little more than one pound;* while the corresponding difference in black children was only two ounces and one-fifth. Of course the number of cases observed is too small to allow a positive conclusion, but it suggests that the difference between the two sexes in the white and in the black races in regard to weight of the new-born is much more marked in the former than in the latter. If the results obtained in these limited observations should be confirmed by more extensive ones, we would have a race distinction which is in perfect correspondence with a known ethnological law.

As to the average weight of the new-born, I may repeat what has been published elsewhere. At my request Dr. Phillips found, from examination of the Philadelphia Hospital records of white children born there, that this weight was seven pounds four and eight-tenths of an ounce. The number from which this result was obtained was one thousand—five hundred males, and five hundred females. The average weight of the males was seven pounds and seven and nine-tenths of an ounce; while that of the females was seven pounds one ounce and seven-tenths.

The average duration of labor in the black women was very nearly fifteen hours, while in the white it was thirteen hours and twenty-five minutes—showing a difference in favor of the latter of more than an hour and a half, that is labor is shorter in the white than in the black women. This result is an unexpected one; nevertheless here again the number of cases is too small to justify a positive conclusion. The duration of labor in white primiparæ was fourteen hours nine minutes; in black, nearly eighteen hours; in white multiparæ, twelve hours forty-two minutes; in black, ten hours sixteen minutes. The duration of the third stage of labor was in the whites twenty-one minutes, and in the colored thirty-three minutes.

And here let me, for the time at least, lay aside these statistics to consider the conduct of the third stage of labor. The subject invites consideration in this paper by the following facts: One of the colored women failing to expel the placenta within an hour after the birth of her child, the gentleman having charge of the

* The difference in the weights of white male and female children is greater than it should be from these facts: first, a larger number of female children; second, in two cases of premature labor and in that of twins the children were females, and their weights being small, of course reduced the average.

case introduced his hand into the uterus and removed the after-birth by piecemeal, or at least the greater portion of it. That patient had septicemia, and infected each of her neighbors; the colored obstetric ward at this time was terribly crowded, the beds so close together that a patient could almost roll from her own bed into the next one.

Shortly after this I was called to a woman in one of the white obstetric wards, who had been delivered of her child three hours before, but the placenta was retained. The patient's pulse was good; there was no hemorrhage, nothing but the simple fact of delay in the third stage of labor. A little friction of the uterus, and compression of its fundus through the abdominal wall, caused the expulsion of the placenta in a few minutes. There was no fragment of the after-birth or of the membranes retained; the genital organs of the patient were not touched either by the *interne* or by myself in this delivery, nevertheless she had septicemia. Finally, a third patient had the placenta retained for nearly five hours, and then it was expelled. She had septicemia. These three patients recovered.

In studying the phenomena of placental delivery we find there are three stages, viz.: First, the separation of the placenta from the uterus; second, its extrusion from the uterine cavity after its conversion into a foreign body by its detachment; and third, its expulsion from the vagina. Delay may occur in any one of these stages, that in the last, of course, being the most easily remedied. The separation of the placenta from the uterus is made by uterine retraction, and probably instead of being marginal in some cases, central in others, is usually general.

A practical question is here presented: Is this separation facilitated by ligating the placental end of the cord; in other words, ought the obstetrician to use two ligatures, or one? The advocates of two ligatures claim that in this case the placenta, being larger, fuller, firmer, cannot so well follow the retraction of the uterus as it can if thin and flexible from the loss of blood, and therefore in the former case is more certainly and completely detached. This is doubted by some, denied by others; nevertheless it seems rational. But admitting its truth, it is certain that if a single ligature be used the placenta is smaller, and hence can pass through a smaller uterine orifice; this practice, no

matter what its effect upon the first, facilitates the second stage of placental delivery.

After uterine retraction has separated the placenta, uterine contractions expel it into the vagina, while the abdominal muscles, aided, it may be, in some slight measure by the contractions of the vagina, cause its final expulsion.

In the spontaneous discharge of the placenta from the uterus, it does not seem yet settled whether the placenta usually presents the foetal surface or the margin at the os uteri. The doctrine of Matthews Duncan has probably for the last few years been most generally adopted by British and American obstetricians; my own belief is that it is correct—at least in some thirty cases of delivery, taking the method advised by Dr. Duncan to test the presentation, I found in the majority that the placenta descended through the os with its margin presenting. French obstetricians have not accepted Duncan's views; and indeed the recent observations of Pinard and others seem to prove that the placenta usually presents by its foetal surface.

Now a practical lesson from this study of the mechanism of placental delivery is, that adopting the view of Duncan, traction upon the cord—a traction which of course is never to be made when the placenta is still attached to the uterus—is mischievous, for it interferes with the normal presentation; but if the normal presentation be that of the foetal surface, such traction facilitates the second stage of delivery.

The time required for the spontaneous delivery of the placenta, as observed by Kabierske in one hundred cases in the Strasburg Maternity, varied from thirty minutes to twelve hours, as is shown by the following table:—

24 times,	.	.	.	30 minutes.
20 times,	.	.	.	1 hour.
25 times,	.	.	.	2 hours.
11 times,	.	.	.	3 hours.
9 times,	.	.	.	4 hours.
5 times,	.	.	.	5 hours.
3 times,	.	.	.	6 hours.
2 times,	.	.	.	8 hours.
1 time,	.	.	.	12 hours.

Few practitioners are willing to trust nature this far, but guard against delay in the delivery of the placenta by following the

uterus down with the hand upon the patient's abdomen, according to the expression and the method of the Dublin school, as the fœtus is expelled, thus keeping the hand upon the uterus at least as a sentinel to warn of uterine relaxation, and, better still, as a stimulus to, and a reinforcement of, uterine retraction. A general observance of this practice reduces to a minimum cases of post partum hemorrhage, of delay in the discharge of the placenta, and of hour-glass contraction.

And now, coming to the practical point of more direct interference with the third stage of labor, what circumstances demand it, and how is it to be made?

I believe the teaching of the Philadelphia school has been favorable to early interference—at least such delay as shown by the Strasburg statistics would not have been allowed by her great teachers. Dr. Hodge advised moderate traction upon the cord at the end of half an hour, or of an hour; and Dr. Meigs stated that he never waited for the spontaneous extrusion of the placenta more than an hour and a half, for he always supposed that if it would not take place in one hour, there was little prospect for its taking place in twenty-four hours. Now, with all reverence for the names of these great men, and with, I trust, due personal humility, it seems to me their teaching was wrong. Even moderate traction upon the cord, if the placenta be attached, is liable to do harm, and traction is not necessary to find out whether it is detached. The statistics quoted prove that one cannot make a time-table for nature in regard of placental delivery—she may effect that delivery long after Dr. Meigs' hour has passed.

As long as the placenta is wholly attached, hemorrhage is impossible; the placenta is still a living structure, and one with the uterus; to tear it loose, to directly detach it from the uterus, opens the way for perilous hemorrhage. Not only this, but such artificial detachment is usually incomplete, is liable to injure the uterine tissue, and the operator's hand may be the bearer of septic germs, or these may pass in with the air admitted during the manipulation, and find a congenial soil for their development in fragments of placenta, or blood-clots that are retained in the uterus. Therefore, unless hemorrhage demands immediate interference, the obstetrician refrains from passing his hand into the uterine cavity for the removal of an attached placenta; a com-

pletely adherent placenta is not so dangerous as the intra-uterine use of the hand for its detachment. I believe, then, that armed expectation is wise in the latter case, only endeavoring, by suitable compression of the uterus with the hand acting through the abdominal wall, to determine or assist that retraction of the organ which is nature's method of separating the placenta. After the detachment of the placenta—a fact which is best learned by feeling a part of the organ with the finger passed into the mouth of the womb—we may, by friction and compression of the uterus, if needed, evoke uterine contractions which will cause its expulsion. Those who believe that the placenta presents its foetal surface at the os uteri, urge the value of moderate and continuous traction upon the cord, thus assisting the moulding of the mass to the orifice through which it is to come. This conservative view as to the management of so-called retained placenta has been strongly presented by Siredey in his recent work upon puerperal diseases. The common expression, retention of the placenta, means very different conditions, each requiring its appropriate treatment.

Passing now to another topic, the relation of acute infectious diseases to the pregnant, or to the puerperal state. The history of the three months furnishes two cases of measles in pregnancy, and one of scarlet fever in puerperality. A report of the latter will appear in the next number of the *American Journal of the Medical Sciences*, and therefore is not presented here. In both the cases of measles the eruption did not appear until after labor, but in each the interval was so short that the disease was present in pregnancy. In one case the disease had no evident effect upon pregnancy, and the puerperal period was normal. But in the other I believe premature labor was caused by the disease, for though no accurate or definite information could be had from the mother as to when the pregnancy began—she was half idiotic—the child was small and feeble, imperfectly developed. Abortion or premature labor is the result in the majority of cases when measles occur in pregnancy. The second patient had septicemia, but even with this complication, and though quite ill, made a perfect recovery.

Puerperal temperature is a subject of importance to which brief reference will now be made. I have here a temperature chart made by Drs. Phillips and Randall, from the charts of

twelve women in whom puerperal convalescence was undisturbed; the chart includes eight days of the puerperal period. The highest temperature was on the fifth day, and then it was only $98\frac{1}{2}^{\circ}$.

Temperature record from two daily averages of twelve cases of normal recovery from labor. The first temperature is that of a woman delivered within the preceding twenty-four hours:—

Morning,	98.4	98.4	98.2	98.2	98.2	98.4	98.0	98.2
Evening,	98.8	98.8	98.8	98.4	98.9	98.8	98.4	98.4

There were opportunities for observing the influence of apparently trifling causes in producing marked elevations of temperature. Thus one patient, whose condition was normal, insisted upon getting up the fifth day and dressing herself; she did so notwithstanding the remonstrance of the nurse, and her temperature rose to a little above 100° . Either from feeling badly, or possibly from the moral influence of the thermometer, she was willing to return to her bed. Another patient, doing well apparently, save that her temperature was 100° , got up the fourth day; her temperature rose to 103° ; she returned to bed; her temperature in a few hours was only 100° , and in two days was normal. In another case an irritant cathartic, or that which proved to be such, the bitartrate of potassium, was given the fifth day, and for a short time the patient's temperature was nearly 105° , but the next day it was normal. On the other hand, the gravity of a case may be much greater than the temperature indicates. Thus in a patient with fatal septicemia the temperature during the first five days only once rose as high as 101° —a part of the time was only 99° —on the sixth day rose to $102\frac{1}{2}^{\circ}$, on the seventh fell to 101° , and then on the morning of the eighth was $103\frac{1}{2}^{\circ}$; she died that day. In the abstract of a paper by Dr. Angus Macdonald, (*British Medical Journal*, May 10), the statement is made that in some of the worst and most rapidly fatal cases of septicemia, the temperature never rose over 101° , if so high. The explanation given was that the vital centres were attacked with such a quantity of the poison that death occurred before the tissue-changes ending in heat took place. Dr. Macdonald further referred to the important difference in the course of temperature in lymphatic and in phlebotic septicemia; there being in the former

a single rigor with sudden and continuous high temperature, and in the latter a series of successive rigors followed by corresponding depressions. Siredey has previously remarked that a temperature chart of a patient having puerperal septicemia, will readily show whether the disease is the lymphatic or the phlebotic form. When Osiander, at the beginning of the present century, and others since him, described remittent puerperal fever, doubtless they had under observation cases of phlebotic septicemia. I am sure these sudden and marked declines of temperature have led practitioners into false diagnoses, especially since attention was redirected by two distinguished American physicians to the occurrence of malarial fever in child-bed; we would much rather believe a patient had this disorder than septicemia, and such desire may assist the diagnostic error, an error I know that I have committed, and I have more than once witnessed its commission.*

The occurrence of a chill at the onset of septicemia is by no means a constant phenomenon. While Dr. Macdonald refers to a chill marking the advent of lymphangitis, Siredey regards it as always present in phlebitis, usual but not invariable in lymphangitis; it is multiple in the former, single in the latter. The cases observed at the hospital show that a chill was not constant in septicemia, even in a fatal form of the disease. While we may in some cases, by the great variations in temperature, be able to diagnose between septicemic phlebitis and lymphangitis, there are decided oscillations in temperature observed in the latter, though much less than in the former; and beside, some cases present the combined forms, lymphatics and veins alike affected. There is herewith presented the temperature chart of a patient who suffered with what I at the time believed to be lymphatic septicemia, and yet the reading of the chart might justify the conclusion that the disease was phlebotic, though early in its manifestation.

* If any one should doubt the difficulty sometimes presented in diagnosing between septicemia and malaria in child-bed, he may be referred to a lecture delivered by Prof. Luigi Mangiagalli upon malaria in its relation with the puerperal state, *Annali di Ostetricia, Ginecologia e Pediatria*, 1883. In this lecture Mangiagalli remarks that in the puerperium, the diagnosis between septicemia and malarial infection is not always easy, that the difficulty may be most grave—almost insuperable.

Bertha Lambert, aged 25; puerperal septicemia:—

DATE.	PULSE.		DAY OF DISEASE.	TEMPERATURE.	
	Morning.	Evening.		Morning.	Evening.
5	—	88	1	—	99
6	64	84	2	98·5	98·7
7	64	112	3	98·6	103 Chill at 3 P. M.
8	96	84	4	100·0	106 Chill at 7 P. M.*
9	100	85	5	100·0	100·5
10	102	114	6	101·7	101·4
11	96	110	7	99·8	100·8
12	100	100	8	102·4	102·4
13	96	96	9	102·8	101·8
14	98	102	10	98·6	100·5
15	80	98	11	97·7	99·4
16	84	98	12	99·4	99·8
17	84	85	13	98·0	99·0
18	88	77	14	97·8	98·7
19	74	72	15	98·0	98·2
20	80	86	16	99·6	100·0
21	78	78	17	100·0	100·4
22	72	85	18	98·4	99·0 †
23	76	68	19	98·3	98·8
24	72	97	20	98·4	98·9
25	82	87	21	99·5	99·2

* Pulse before chill, temperature afterward. † Child died of pneumonia.

Looking at it one sees that the temperature was normal until the morning of the third day, when the first chill occurred, at that time it rose to 103°; the next day a chill in the evening, and the mercury marked 106°, but fell the next morning to 100°; the next most marked difference was observed on the ninth and tenth days—the evening of the former it was 101½°, the next morning 98½°. I show a second temperature chart of a patient whose temperature was under 100° until the fourth day; was 104½° the seventh day, dropping to 99½° the eighth day; reached 105° on the eleventh day, the twelfth only 101½°; and who had in the course of her illness at least two chills.

Kate Fleming, aged 22, puerperal septicemia:—

DATE.	PULSE.		DAY OF DISEASE.	TEMPERATURE.	
	Morning.	Evening.		Morning.	Evening.
10	88	88	2	98·0	98·0
11	90	92	3	98·9	98·0
12	82	84	4	98·2	99·6
13	108	104	5	100·6	102·2

DATE.	PULSE.		DAY OF DISEASE.	TEMPERATURE.	
	Morning.	Evening.		Morning.	Evening.
14	96	112	6	101·5	103·0
15	124	106	7	104·4	102·8
16	81	90	8	99·6	103·3
17	80	88	9	100·4	101·8
18	74	88	10	99·2	101·2
19	116	120	11	103·0	105·0
20	104	98	12	101·4	101·3
21	90	106	13	103·0	101·3
22	94	112	14	100·3	104·0
23	98	98	15	100·8	100·2
24	84	82	16	99·0	98·8
25	93	87	17	97·2	98·8
26	79	72	18	98·0	98·2
27	67	70	19	97·8	97·4
28	63	78	20	97·2	98·2
29	80	62	21	98·0	98·5
1	64	72	22	97·6	98·4
2	66	92	23	97·8	99·3
3	80	—	24	97·0	—

The cases of septicemia were too few, and the discrimination between lymphangitis and phlebitis not always made, to permit me to give a positive opinion; nevertheless, it seems to me probable that in lymphangitis the oscillations of temperature are always such that the thermometer marks a higher degree in the evening; while in phlebitis, the highest temperature occurs quite as often in the morning as in the evening.

Returning to the subject of normal temperature in puerperality, it will be seen from the chart presented that the temperature of the third was no higher than that of the first or of the second day. In looking at a temperature chart given by Dr. Macdonald (Edinburgh Obstetrical Transactions, vol. vi), taken as the result of observing the temperatures of thirty women, I find the highest temperatures the third, fourth and seventh days; the thermometer registered $99\frac{1}{2}^{\circ}$ the third day, and $99\frac{1}{2}^{\circ}$ the fourth and seventh days.

Tarnier remarks that momentary elevations of temperature do not generally involve an unfavorable prognosis; but when they are progressive and continuous, especially when the thermometer placed in the axilla goes above $100\frac{4}{10}^{\circ}$, some complication is to be feared.

One of the subjects delivered at the hospital had a slightly subnormal temperature. She was a girl, eighteen years of age, who, three hours after a normal labor, had a temperature of 99° ; this fell so that on the third day it was only 98° , and so continued for a week. During a part of this time her pulse was 56, and even only 48.

The presence of albumen in the urine of the pregnant woman has often, even generally, engaged the attention of obstetricians; but comparatively little concern is usually shown as to its presence during labor, or in the puerperal state. Possibly, it may be quite as important to examine the urine of the lying-in as of the pregnant woman, especially if she has had even slight septicemia.

But first, how frequent is albuminuria in pregnancy? In seventy-two pregnant women albuminuria was found in five. It will be observed that this proportion is very much less than that given by Charpentier,* quoting Dumas, who, combining the statistics of several observers, makes the proportion one to five or six. It seems to me, both from hospital statistics and from observations in private practice, this proportion exaggerates the frequency of the accident.

By the albuminuria of labor is understood not only the disorder as occurring during labor, but also that of the two or three days immediately preceding. This is very much more frequent than the albuminuria of pregnancy, but the cases examined with reference to this point were too few to determine the proportion.

Seven of the seventy-two women had albuminuria after labor; I think the number was much greater, but some of the women suffering with septicemia did not have the urine examined until after convalescence, and the results of examinations made in others were not properly kept, or at least were not placed in my hands.

In three of the seven mentioned the albuminuria was slight and transient. In four women convalescing from septicemia, the urine was found to be albuminous one month after delivery. Two had pus, blood and hyaline casts in the urine; in a third, no pus, but blood and casts were present in the urine; as to the

* *Traité des Accouchements.*

urine of the fourth, the microscopic appearances were not noted. In regard to two of these patients, I know that the catheter was first used after their being brought from the "fever" to the "convalescent ward," and therefore the explanation which Olshausen has suggested of the renal disorder fails in these cases—catheterism had nothing to do with its causation. In explanation of these cases, it is probably better to accept the teaching of Siredey, who regards puerperal nephritis as a constant complication of uterine lymphangitis or phlebitis.

Women may apparently, but not really, recover after pregnancy and labor; especially if there has been septicemia, is there a liability of renal disorder becoming chronic, and it is only by actual examination of the urine that the integrity of the kidneys can be determined.

Mauriceau compares the pregnant woman just before labor to a ship that has been nine months tossing upon a rough sea, and urges the importance of not letting the ship sink as she enters the port of child-bed. It is not less the duty of the obstetrician to know that the ship has not suffered such damage on the ocean or in the port, that she is unfit, without important repairs, to run the risk of another voyage.

Sugar in the urine of pregnant and of nursing women was first shown to occur by Blot in 1856. Differences of opinion hold as to the constancy of its presence in the conditions stated, as to its source and as to its character. Macdonald found it in each of thirty-five cases whose urine was examined, and therefore regards it as present in all cases at some time or other of the puerperium. But neither Kleinwächter nor Spiegelberg refers to it as always present. In the examinations made daily of the urine of fifty women at the hospital (these examinations began a few days before and continued seven days after labor), four women had sugar in the urine before labor, and six after labor, one of the six being also one of the four. In this woman the sugar was constantly and largely present up to eight weeks after delivery; she had remarkably well-developed mammary glands, and a most abundant secretion of milk. In this case Blot's suggested test for a good nurse—to wit, the quantity of sugar contained in the urine—would have proved true, so far as abundance of milk was concerned.

It has been shown that abrupt suppression of nursing causes the appearance of sugar in the urine; thus it is commonly observed in mammary abscess.

The fact that removal of the mammary glands in an inferior animal recently delivered, causes disappearance of sugar from the urine, proves that it is incorrect to call the cases where sugar is found in the urine in pregnancy or child-bed, cases of glycosuria, but rather of lactosuria, unless we attach only the literal meaning to the first word in the compound glycosuria. Spiegelberg refers to the condition as an absorption diabetes; and this seems the opinion of most authorities. Tarnier, however, regards as very plausible the hypothesis that the sugar eliminated by the kidneys was sugar made very probably by the liver in view of the lacteal secretion, and which was not utilized in consequence of the momentary suppression of this function; further, he thinks new researches necessary, in addition to those of Hofmeister and others, to determine the question as to whether this sugar is glucose or lactose.

Whenever there is an exact correspondence between the milk supply and the demand, the former not being in excess of the latter, it is probable sugar will not be found in the urine; I think, therefore, that the experience of Macdonald—showing saccharine urine in all cases of lying-in women—is not the law.

An interesting case of secondary puerperal hemorrhage occurred—interesting as to its etiology, and instructive as to the means by which it was finally arrested.

The following is the history as given by Dr. Voorhees, the *interne* who had charge of the patient:—

A. A., German, single, primipara; varicose condition of veins of lower limbs, this condition disappearing after labor. Labor at full term, March 5, 1884, lasting a little over twelve hours. Her condition was perfectly satisfactory up to the evening of the eleventh day after confinement; on that day she was transferred to the convalescent ward, and then saw the out-door agent as to keeping the father of her child in prison for refusing support. She was greatly distressed by this interview, and at 4.30 the next morning hemorrhage began. Digital examination showed that the blood came from the uterus; the os was high up, flabby and full of clots; the uterus was as large as if delivery had just occurred, and was soft and relaxed. Ergot was given; the child applied to the breast; the uterus was emptied of its clots, and friction used to stimulate contraction, but the

bleeding still continued. Ice was then used to the abdomen, and in the vagina; the bleeding was not stopped. Hot water was then freely thrown into the uterus and the result was prompt and satisfactory. The patient made a good recovery. Although the uterine discharges were carefully examined, at no time was there any organized material found, nothing in the least indicating that this hemorrhage was caused, for example, by the retention of a placental fragment.

Those who have read Dr. Fordyce Barker's admirable lectures upon puerperal diseases, will remember the graphic description of a case of secondary hemorrhage the second day of lying-in, caused by an emotional cause, and in what perilous condition the poor woman was for some days. So too in the hospital case we have an example of hemorrhage from a psychical cause. Believe or doubt as we may, say what we will, there are at times in medical practice just such sudden, startling and strong proclamations of something more than flesh and blood in this human nature, telling us that the coarse material may be prostrated through the finer spiritual, the psychical assert its power over the physical.

Further, as to this case, the great value of hot-water injections for the arrest of uterine hemorrhage never had a more striking illustration.

The final subject presented to you is that of uterine rupture. In reflecting upon the history of my three months' service, no event occurred in my duties to these unfortunate women—women often worthy of the profoundest pity as the victims of misfortune, and of man's perfidy—which causes me greater sorrow in silence or in recital than a case where the uterus was ruptured in consequence of a shoulder presentation, a case which ended in death the eighth day after delivery. Yet I would fail in duty to my profession that has been so good, so generous to me, if I did not make the case fully known. The patient was a well-formed healthy multipara; she had been in labor nearly twelve hours when I first saw her, the left shoulder presenting. Ether was immediately given until she was thoroughly under its anæsthetic effect; and then, without violence, nay, with great ease, I passed two fingers behind the right knee, brought the foot down, and turning and delivery were effected in a few minutes; the placenta followed almost immediately; the child, quite a large one, was dead. The patient came out from the anæsthesia satisfactorily;

her pulse was good; there was no complaint, no shock, no great hemorrhage. Yet that woman had a ruptured womb, the tear beginning at the os uteri on the right side, involving the cervix and the lower part of the body of the uterus, this condition being made known by the post-mortem. If it be thought I ought to have known this accident at the time of delivery, I can only say that like ignorance happened to Dubois, to Hervieux, to Tarnier, and others—the first revelation of the uterine rent being made at the post-mortem; these silent tears of the womb are, as Hervieux has suggested, probably more frequent than generally thought. No, my self-reproach is not in this, but in not having made myself, or by another, an examination during pregnancy, so that the abnormal presentation could have been corrected, if not then, at least early in labor. But let this pass. The great practical lesson to be drawn from the accident is not only the importance of an early rectification of a mal-presentation, but also an appreciation of the danger of rupture of the uterus, and how this accident occurs. The drawing now shown gives the position occupied by the child, and also and especially gives the change in form and thickness of the two cavities of the uterus, which, as so admirably described by Bandl, are formed when nature is unable to overcome the obstacle to labor found in such case. The one cavity is formed by the body of the uterus, and its walls become thicker and stronger; the other, by the cervix, and its walls grow thinner—become indeed so attenuated and weak that a very slight additional strain causes a tear at some point; that strain may come from a uterine contraction, or solely from the introduction of the finger: and thus peril from action, peril from delay must be before the obstetrician's mind when called to a case of neglected shoulder presentation.

Of course had I seen this patient an hour or two earlier, the event might have been different. The pressure of the presenting part had been so severe that a slough of the vesico-vaginal wall occurred, and the patient, had she recovered, would have required an operation for the resulting urinary fistula; I have thought that possibly the uterine rent was in part the result of a slough also; but be this as it may, there was not the slightest indication given at the post-mortem that any hemorrhage in the abdominal cavity had taken place.

One other topic I had designed presenting, the prophylactic treatment of puerperal septicemia, but my paper has already occupied enough, possibly too much, of your time.

1902 CHESTNUT ST., PHILADELPHIA.

DISCUSSION ON OBSTETRIC REPORTS.

DR. ALBERT H. SMITH, in opening the discussion, said : I feel unable to discuss this paper thoroughly, but can say that it is just such papers that are the most important contributions to medical science, containing, as it does, a record of observed clinical facts, by a competent observer. The statistical portion of the paper is interesting, but offers no field for discussion. I agree with Dr. Parvin that the teaching in Philadelphia is in favor of early interference in the third stage of labor, but there is an exception : Dr. Joseph Warrington, who had charge formerly of the Philadelphia Lying-in Charity, which is now under my charge, taught that as long as there is complete attachment of the placenta there can be no hemorrhage, and no danger therefore ; if there is no bleeding, there is no detachment ; we were taught, that there was to be no traction on its cord, but it was to be used as a guide and by it traction was to be made on the foetal surface of the placenta. The placental mass will, by the twisting, become a small cord. The detachment of the placenta may be marginal or complete. It differs in different cases. Mathews Duncan's position is not tenable. Dr. Parvin will probably bear out the statement that foetal surface often presents. I am pleased to hear of Dr. Parvin's success with hot-water injections ; they give us a very simple means of controlling one of the most terrible accidents of parturition ; they can be used by a tyro or a nurse, and are much preferable to the powerful astringents used by the English practitioners.

I am also glad to see that Dr. Parvin has had the courage to report the case of uterine rupture. He sets a good example. This case was carried through with every precaution, and shows that the accident may occur in the best hands ; in fact, it is a wonder that rupture does not occur oftener ; any one who has examined women by abdominal palpation, will remember how thin the uterine walls are at full term.

DR. PARISH : Dr. Parvin deserves great credit for the careful collection of facts reported ; but it is not possible at present to discuss the points involved in the figures presented. I would like him, in his closing remarks, to answer more fully the question in regard to the time of delivering the placenta. The general teaching is that the placenta should be delivered in the first half hour, if necessary, by the introduction of the hand into the uterus. I followed this practice at one time, but I should now wait much longer before introducing my hand into the uterine cavity, unless hemorrhage, otherwise uncontrollable, should occur. It is exceedingly rare that the placenta cannot be removed with sufficient prompt-

ness in some other manner, I am averse to the introduction of the hand into the uterus.

The author of the paper, however, seemed to leave his hearers under the impression that he would leave the placenta in the uterus an indefinite period, *i. e.*, until it came away itself, rather than introduce the hand for its detachment. I do not consider it safe to leave the placenta very long. I would not leave the house until the secundines had been entirely removed. The placenta may become detached and occasion dangerous hemorrhage in the absence of the physician. Decomposition and blood-poisoning may occur. Decomposition may occur in twenty-four hours. One cannot wait with safety for hemorrhage as an indication for removal. Fatal blood-poisoning has occurred because of the adherent placenta without hemorrhage even appearing. I have seen in consultation four mothers, who died because of placenta, or portions of it, being left in the uterus. They were patients of members of this Society. I have never dared to leave any portion of the placenta in the uterus, and I may add that I have never lost a recently confined mother in my private practice.

To remove adherent portions of placenta, I have, in a few instances, with complete success, injected hot water into the uterine cavity. It both detaches and expels the mass by its effect in securing uterine contraction. I was glad to hear the remarks on septicæmia, notably in reference to the absence of chill. I have repeatedly seen in the Philadelphia Hospital, cases in which chill did not occur, the disease coming on insidiously, and even proving fatal.

I have seen cases in which the rise and fall of temperature had misled the immediate attendants into supposing the condition to be malarial. Should the fall occur after the use of antipyretics, the fall might be ascribed to the remedies used.

The responsibility for the uterine rupture reported should not rest on Dr. Parvin. The patient, I have learned, had been in labor for some hours, and the damage had doubtless occurred before the arrival of Dr. Parvin. Version had been attempted without anæsthesia.

It is a cardinal rule with me never to attempt version by the hand in the uterus without full anæsthesia.

DR. J. M. KEATING: Last year I had occasion to examine carefully the records at Blockley of the weight of new-born children. There were many hundreds of cases, covering a period of over twelve years. It was an interesting fact that there was no record of a child weighing over eleven pounds. To be sure, illegitimacy may have something to do with it; nevertheless, probably one-third of the mothers are married women. The mothers, as a rule, are strong and healthy, and probably most of the fathers belong to the laboring class, and I can only account for the low average in weight by the diet of the mothers during gestation. If low feeding will influence the weight of the child, could this be taken advantage of to increase the vitality in certain cases? Dr. Parvin's averages are probably less than would be those of private practice where twelve-pound children are not very uncommon.

As regards the high temperature of the recently confined women, and the epidemics of so-called puerperal fever, the matter has been much discussed of late. I believe that there is much irregularity in the characteristics of these temperature records, due, in all probability, to a variety of causes. It is a mistaken idea to believe that every woman confined at Blockley had a record of high temperature. There are times when a large number of cases present no irregularities whatever, and yet a microscopic examination of the air has shown it to contain the greatest amount of impurities and bacteria, according to reports made by Dr. Formad. Many of the most fatal epidemics of puerperal fever, and it occurs in epidemic form, have been initiated by scarlet fever. Their visit has been sudden, and their termination abrupt. The presence of measles in epidemic form amongst the children in the house, has always been followed by high temperature in the puerperal women. It may be recorded that not long since Dr. Formad investigated for Dr. Keating the blood of some children with malignant measles, and studied carefully the micrococcus therein found. Shortly after, the blood of a woman then very ill with puerperal fever, was examined, and her blood was found to present the same characteristics as were noted in the measles cases; the white blood-corpuscles were soon attacked, and on that account an unfavorable prognosis given. The woman finally recovered. Is it not possible that this case was suffering from some blood-poisoning due to infection from measles?

Unfortunately, these studies were interrupted, but they will be again resumed. There may be several forms of blood-poisoning inducing these high temperatures, as septicaemia from putridity, from the reabsorption of decomposing matters, or a blood-poisoning from germs of infectious diseases, which may gain entrance through the respiratory tract, as do measles and scarlatina.

DR. LONGAKER: I have had the misfortune of seeing two cases of rupture of the uterus. The last one occurred about one year ago. I was called by a midwife to assist in the delivery of a case under her care, a presentation of the breech. The patient was a multipara about 38 years of age, a German, and of large frame. The labor was not a severe one, and no difficulty was experienced until the shoulders were about to be delivered. The pains were inefficient. Ergot was given, and by means of traction she succeeded in delivering a still male child of large size, some time before my arrival. Immediately following the birth of the child, I was told, there occurred a profuse hemorrhage which was followed by syncope, from which she had recovered. The placenta she failed to deliver. Carefully following the cord it was found presenting at the os, and without any difficulty it was removed.

Following it came a profuse flow of clots and uncoagulated blood. Ice and hot water were alternately carried into the uterine cavity, and firm contraction of the uterus secured. In spite of this there continued a free loss of blood. Again passing my hand into the vagina an extensive rent was detected; it involved the posterior right aspect of the cervix, extending into the lower segment of the uterus and involving also the upper end of the

vagina. My hand easily passed in so that the tips of the fingers of the right hand could be brought into contact with the left external hand, with nothing but the abdominal parietes intervening, and below this could be felt the uterus. Hemorrhage continued, she passed into a state of deep collapse, and within an hour she was dead.

The other case was seen when a pupil physician in the Phila. Lying-in Charity. When I reached her she had been in active labor, with rupture of membranes for four or five hours. It was her ninth labor at full term.

The presentation was of the face with chin posterior.

The pains grew more violently expulsive and, though neither ergot was given nor were efforts made at version there was an extensive rupture of the uterus; with it, cessation of all contractions and profound collapse rapidly came on. In about an hour from this she had recovered partially, but the same state grew again alarmingly deep, with efforts which were made at delivery, which were also accompanied by a free external hemorrhage. Here death took place about four hours after the occurrence of rupture.

In both of these cases the symptoms and effects of this accident were markedly different from that of the case presented in the paper to-night by the author, Dr. Parvin.

DR. MONTGOMERY: I am pleased with the instructive lessons gleaned from Dr. Parvin's term of service and cannot but feel that it reflected upon those who like himself had been so long connected with the institution, without arriving at any deductions of special importance to the profession. I am disappointed that he had omitted from his report, an analysis of the cases of septicemia, in which the term had afforded considerable experience. It was a subject at present exciting profound thought and discussion as to its nature and etiology. I would infer from the paper that Dr. Parvin believed it due to bacteria or a micrococcus, the same as produced septicemia in any wound. This view was confirmed by the method of treatment, in which his results were unusually favorable, but as the author has not alluded to this in his paper, I do not feel that it is just to enter upon its discussion.

DR. W. T. TAYLOR: I believe I have attended several hundred labors and I have never seen any harm result from removing the placenta. It has served its purpose when the child is born and therefore is a foreign body. After the fetus is expelled, I place my hand on the abdomen to secure uterine contraction and if in fifteen or twenty minutes the placenta does not come away, I gradually insert my fingers and hand—if required—to loosen and remove it. Sometimes I use ergot, but I never wait more than a half-hour.

DR. PARVIN: My remarks must necessarily be brief. As to Dr. Allen's preference for embryotomy rather than turning in a shoulder presentation, as being safer for the mother, I am inclined to believe the practice he advises is right in case the child be dead.

Dr. McFerran has suggested that we can turn without the use of ether. This is true; and we can also walk to New York, but we don't do it: version

can be accomplished more readily, and more safely if an anæsthetic be used. The question has been asked as to how long I would wait for the placenta to be expelled. We should bear in mind there are three stages in placental delivery: first, the detachment of the placenta from the uterus; second, its expulsion from the uterus, and third, its expulsion from the vagina. The gentleman who spoke of the large number of times he had removed the placenta by the hand, was in at least almost all cases dealing with the second or third stage, and so, insomuch as such cases are concerned, justly regards the matter as harmless. It is the detachment from the uterus that is dangerous—then the placenta is one with the uterine tissue, and the tearing it loose by the hand in the uterus cannot but be dangerous. In this first stage of placental delivery, I would wait several hours before resorting to direct detachment. The introduction of the hand into the uterus after the expulsion of the child is a much more serious matter than its introduction before that expulsion, for in the former case there are abraded and torn surfaces ready to receive septic poison.

I would not give ergot, because imprisonment of the placenta would be liable to occur.

Absorption of the placenta can hardly be admitted after Hegar's studies of the subject. Nor does the past experience of the profession justify the hope that by a limited diet, or by medicines given a pregnant woman we can hinder the development of the fœtus, causing its growth to be so retarded that labor will be made easier or safer.

I have been asked as to puerperal septicemia. That is too large a subject for present discussion. But so far as its cause is concerned, let me say that I believe it to be a single poison with many manifestations, as specific as is the poison of scarlet fever; nothing but it, though the medium in which it is may vary, can produce puerperal septicemia, and puerperal septicemia can produce nothing else. This is my creed, my belief, not my knowledge.

REASONS FOR BELIEVING IN THE CONTAGIOUSNESS OF PHTHISIS.

Read June 11, 1884.

BY W. H. WEBB, M. D.

THE germ theory of disease is by no means a new theory. One of its earliest advocates was Athanasius Kircher, a learned German Jesuit, who lived in the early part of the seventeenth century; and about the same time lived Robert Boyle, an eminent Irish philosopher, who believed in the truth of this theory.

The renowned Linnæus, the father of botany, was not only an ardent investigator of its claims, but published several memoirs in its support. In the latter part of the last century it had such supporters as Sir John Pringle and Dr. Wm. Farr, and in the early part of the present century it had such advocates as Sir Henry Holland, Schonlein, Cagniard de la Tour, Schultze and many others.

To the illustrious Pasteur, however, belongs the distinction of having done more than any of his predecessors to develop this intensely interesting and important subject, and of presenting its truths in such a way that they have become of immense practical use to mankind. His indefatigable labors, the ingenuity and exactness with which he pursued his investigations, the practical demonstrations he has given of the utility of the truths he has discovered, are well known to men of science. This special field of investigation is now occupied by many able and accurate observers and as a result of their labors the present generation may witness an epoch in the history of medicine, startling in its brilliancy of grand achievements in subjugating disease; when men shall hold in their hands effective weapons and be enabled to erect impassable barriers before those destructive scourges—cholera, yellow fever and tubercular phthisis. But two years ago the medical profession was startled by the announcement made by Robert Koch,* of Berlin, that, "Tuberculosis is a specific infectious disease, caused by a specific micro-organism, the bacillus tuberculosis, which constitutes, in fact, the tubercle virus." Truly, since the time of the immortal Jenner we have not had such a remarkable statement, nor one so weighty in its import! There is no reason to wonder why medical and other scientific bodies, the world over, have this subject so frequently under consideration.

The work of Dr. Formad, of this city, as well as the work of all other investigators in this field of research, has seemed to confirm the statements of Prof. Koch. There is nothing more seductive than speculation regarding the outcome of their future labors, of the truths which shall belong to those who will follow us, and of the beneficent power these truths shall arm them with.

But it is only hard and intelligent work that can make this

* Die Etiologie der Tuberculose. Berliner Klin. Wochenschrift, 1882, No. 15.

future a reality, and I am persuaded that the frequent discussions by our Society, of points connected with the study of germs as a cause of disease, will do much good by stimulating the efforts of those members who are engaged in such investigations, and by enabling others who are interested in this work to make suggestions or offer criticisms that may be of some advantage to them.

In my paper this evening, I purpose to limit myself to answering in the affirmative, a question of great practical importance propounded by Dr. Formad in his recent paper, namely:—"Is Consumption Contagious?" Dr. Formad is disposed to answer it in the negative and offers to you the names of a number of eminent physicians who apparently lend strength to his doubts concerning its contagiousness. For a number of years I have carefully studied this disease, and as a result of my observations I am firmly convinced that it is contagious. Indeed, the contagious character of the disease is generally believed in, and is taught by the most able and experienced clinicians of our day.

When a disease is unusually prevalent, it is very natural to suppose that it may be due to contagion or infection. Think, for a moment, of the ravages from tubercular phthisis. It exists in all climates; it affects all classes of people; it respects neither age nor sex. It claims about twenty per cent. of the death-rate of the civilized world. The mortuary lists of our own city show a percentage in its favor amounting to about fifteen and a half, and the native population of those latitudes most frequented by consumptives succumb to this dire disorder as frequently as people do elsewhere! except, perhaps, Colorado. Is this to be accounted for by heredity or pre-existing lung trouble? If the disease was due to inheritance alone it would have become obliterated generations ago by a species of natural extinction, but the disease is increasing in a greater ratio than the increase of population, which shows that the disease *must be acquired afresh*.

Dr. Formad, in his valuable paper, makes the statement that, "According to the observations of the most prominent clinicians who have paid special attention to this matter, there is not a single authenticated case of tuberculosis as a result of contagion on record." This assertion is not tenable, since cases are recorded

by C. B. Coventry,¹ S. G. Morton,² Daniel Drake,³ Tauchard,⁴ H. G. Bowditch,⁵ Vialettes,⁶ Beregeret,⁷ Hardy,⁸ Seux,⁹ Condie,¹⁰ L. Tait,¹¹ Stevens,¹² Bernard,¹³ Chamontin,¹⁴ Herman Weber,¹⁵ Flint, Sr.,¹⁶ Holden,¹⁷ Reich,¹⁸ Da Costa,¹⁹ Booth,²⁰ Bryhn,²¹ and many others. Is this not sufficient evidence that such cases are recorded? The fact that some of these names are better known than others, does not militate against the honesty and care exercised by the less distinguished observers and their deductions, and are justly entitled to a fair consideration. Obscurity does not, by any means, imply ignorance, lack of ability and keen perception.

Dr. Formad also asserts that, "Among scores of experienced men who deny thus the contagiousness of tuberculosis, it is sufficient to mention the names of Virchow, Recklinghausen and Stricker, in Germany; Gull, William Watson, Paget, Humphrye and Richardson, in England; Bennet, in France, and Hiram Corson and Trail Green in our midst—all men of close observation, with ripe experience extending over from thirty to fifty years." I also take exception to this statement, for Drs. Corson, Bennet, and probably many others mentioned in Dr. Formad's list, if heard from to-day, would not subscribe to this declaration, which finds fewer supporters than one might imagine to be the case. Some time ago I received a letter from Dr. Corson, in which he said: "Long since I advised my patrons not to have young daughters, who were compelled to wait on a consumptive mother, sleeping in the same room with the patient." This certainly shows that while

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1. U. S. Med. and Surg. Journal, New York, 1835, p. 392.
 2. Illustrations of Pulmonary Consumption, Phila., 1837, p. 80.
 3. Principal Diseases of the Int. Val. of N. A., Phila., 1854, p. 915.
 4. These de Paris, 1860, p. 37.
 5. Boston Med. and Surg. Journal, 1864, p. 329.
 6. These de Montpellier, 1866, No. 44.
 7. Annales d' Hygiene et de Medicine l'egale, 1867.
 8. Bulletins de Academie de Med., 1868, p. 348.
 9. La Marseille Medical, 1869, No. 4, p. 310.
 10. Am. Journ. of the Med. Sci., July, 1871.
 11. Ibid., Oct., 1871.
 12. Boston Med. and Surg. Journ., 1872, p. 168.
 13. These de Montpellier, 1872, No. 46.
 14. Ibid., 1874, No. 22.
 15. Clinical Societies Trans. London, 1874, vol. viii, p. 144.
 16. On Phthisis, Phila., 1875, p. 419.
 17. Am. Journ. of the Med. Sci., July, 1878, p. 145.
 18. Reynolds' System of Med. Am. Ed., 1880, vol. ii, p. 117.
 19. Am. Journ. of the Med. Sci., April, 1878.
 20. Trans. of Southern Ill. Med. Assoc., 1879.
 21. London Med. Record, 1880.

Dr. Corson may not be a thorough convert to the contagion theory, yet he thinks it prudent to resort to preventive measures, by securing as much separation as possible of the well from the phthisical individual. And Dr. Bennet, who is also quoted by Dr. Formad, records the following typical case of contagion in his work :*

"A strong, healthy, well-made husband, age 27, with no hereditary or constitutional taint or weakness, came over from Australia—a four months' journey—in the same cabin as his wife, who was in the last stage of suppurative phthisis. She died soon after her arrival in England, and he came to Mentone that winter a confirmed consumptive, dying himself subsequently. He was perfectly well when he stepped on board of the vessel at Australia ; but in a small confined cabin breathed for months an atmosphere loaded with pus particles thrown out of the suppurating cavities of his wife's lungs, possibly to his destruction."

After referring to the inoculating experiments of Buhl, at Munich, he makes the following statement.—"But in the face of the results that these researches have brought to light, *it seems to me impossible to deny that it may be communicated to the healthy by breathing constantly air saturated with the purulent secretions of advanced phthisis.* This is an all-powerful argument [I am still quoting Bennet] for the free ventilation of rooms occupied by the consumptive, for the sake of those who attend them and live with them, as well as for their own. In a confined atmosphere they probably poison themselves by their own foetid breath, and extend disease to the healthy regions of the lungs."† Can this be used to confirm the belief of Dr. Bennet in the non-contagiousness of phthisis, or is it evidence in support of the position I take? This is but one of many similar cases quoted by various authors, who are scarcely willing to commit themselves while the evidence is so striking, that they, like Dr. Bennet, feel constrained to express the possibility of a contagious element in their causation.

"Whatever has happened, is capable of happening again ; the only question relates to the condition under which it happens,"‡ and what are the conditions under the present circumstances? This is exemplified in the following case, very similar to the one narrated by Dr. Bennet :

* On the Treatment of Pulmonary Consumption, Phila., 1879, p. 51.

† Ibid, p. 53. Italics mine.

‡ J. S. Mill, System of Logic, 9th Ed. London, 1872, vol. 2, p. 144.

"A lady, about 30 years of age, the wife of an officer in the army, left Calcutta with her husband to go by sea to Southampton. At the time of leaving Calcutta *she* was in *robust health*, whilst he was in an advanced stage of consumption. They had a single close cabin, and she performed all the duties of a nurse for her husband. The weather was stormy, and the hatches were more than once battened down. The husband died off the Cape, and was buried at sea. About three days later the lady arrived at Southampton. I was called to see her professionally. I found her with both lungs stuffed with tubercles; and she died in about six weeks afterwards. The painful duty was cast upon me of acquainting her with her condition, which I did, when she said 'Impossible; I was never better in my life than when I stepped on board at Calcutta.' I knew the lady well, and all her family, and there was no hereditary predisposition. In this case, all the necessary conditions for the propagation of the disease were fulfilled; a *high-temperature* in a close ill-ventilated cabin, where the exhalations from the diseased lungs were inhaled by the sound lungs, with the well-nigh inevitable result I have described." *

Were these the only cases we would think our point sustained. There are, however, but few physicians in our large cities who cannot recall cases in which they would rather commit themselves to a belief in the contagiousness of consumption than to ascribe its cause to anything else.

The following cases, due to contagion, have been communicated to the writer and are worthy of record:—

Dr. J. Solis Cohen has kindly furnished me with following case:—

"More than ten years ago Dr. H. of this city, sent me a young female from the country in advanced phthisis, much emaciated, with aphonia from pressure of consolidated apex on right recurrent nerve. I gave her little encouragement and sent her home with some general instructions. Some two years later she called on me asking me if I remembered the last words I said to her; I did not, but she repeated them, 'Your best chance is to take cod-liver oil, live on it if you can, eat it with your bread, with anything.' She went home much depressed, became bed-ridden for a number of weeks. She subsequently married, her husband acquired phthisis and died of it, and she was still living three or four years ago with consolidated apices and cicatrized cavities."

Juan B. Mears, of Monterey, Mexico, communicates to my friend, Dr. A. C. W. Beecher, of this city, the following case:—

"A woman, suffering from the last stages of consumption, who some year or two before had adopted a girl. The latter's mother, Mrs. H., questioned me about the safety of letting her daughter remain with the afflicted person. I examined the girl and found her strong and healthy, tall and

well developed for her age (about 16 or 17 years old), without any hereditary predisposition, I told her that I feared no danger to the daughter if she would only take some ordinary hygienic precaution. Well, the old lady died, and a few months afterwards Mrs. H. brought the girl to me, already with an as acute phthisis as I ever saw, and she died too. Questioning them about the case, they attributed the malady to the companionship of the old woman; they slept in the same bed, ate from the same dishes, breathed the same air, infected probably by the phthisical debris. I must add that the people here believe in the actual contagiousness of phthisis."

The following case occurred in my own practice, and is also worthy of record :—

October 7, 1880, I was requested to attend professionally Mr. H. F., aged 52 years, who had been ill for one year with phthisis. He died September 23, 1881. During all of Mr. F.'s illness he was most carefully nursed by his wife, who occupied the same room constantly; she contracted phthisis, and died August 3, 1882, aged 50 years. Mrs. F.'s mother died twenty years previously of phthisis, her father died at the age of 56 of cancer.

I may be permitted, in view of the few eminent names offered by Dr. Formad in support of his theory, to mention the names of a number of men of equal practical experience in medicine, who have recorded their belief in the contagiousness of the disease :—Aristotle,¹ Galen,² Riveris,³ R. Morton,⁴ Baume,⁵ Cullen,⁶ Herberden,⁷ Darwin,⁸ Coventry,⁹ S. G. Morton,¹⁰ Bright and Addison,¹¹ Dunglison,¹² Andral,¹³ Drake,¹⁴ Sir T. Watson,¹⁵ Copeland,¹⁶ Dickson,¹⁷ W. Budd,¹⁸ L. Tait,¹⁹ Walshe,²⁰ Madden,²¹ de Mussy,²² H.

1. Practical and Historical Treatise on Consumptive Diseases, by T. Young, M. D. London, 1816, p. 15.

2. Paulus Ægineta, Syd. Soc. 1844, vol. i, p. 286.

3. Practice of Physic. London, 1668, p. 170.

4. Phthisiologia, or a treatise on Consumption. London, 1694, p. 67.

5. Phthisis Pulmonaire. Montpellier, 1789, vol. i, p. 189.

6. Practice of Medicine. Edinburgh, 1790, vol. ii, p. 390.

7. Commentaries on the History and Cure of Disease. London, 1802, p. 375.

8. Zoonomia. Phila., 1818, vol. i, p. 311.

9. U. S. Med. and Surg. Journal. New York, 1835, vol. i, p. 389.

10. Illustrations of Pulmonary Consumption. Phila., 1837, p. 80.

11. Elements of the Practice of Medicine. London, 1839, vol. i, p. 294.

12. Practice of Medicine. Phila., 1844, vol. i, p. 365.

13. Notes to Lænnec's Treatise on Auscultation, edited by Herbert. London, 1846.

14. Principal Diseases of the Int. Val. of North America. Phila., 1854, p. 915.

15. Principles and Practice of Physic. London, 1857, p. 217.

16. Dictionary of Practical Medicine. New York, 1859, p. 1228.

17. Elements of Medicine. Phila., 1859, p. 625.

18. The Lancet, 1867, vol. ii.

19. Amer. Journal of the Medical Sci., 1871, vol. ii.

20. Diseases of the Lungs. London, 1871, p. 452.

21. Dublin Journal of Med. Sci., vol. xl, p. 33.

22. Brit. and For. Med. and Chir. Rev., April, 1870, p. 529.

Weber,¹ Holden,² Da Costa,³ Rühle of Bonn,⁴ Lichtheim,⁵ Klebs,⁶ Bollinger,⁷ Flint,⁸ and many others could be mentioned.

Dr. Formad lays great stress upon the fact that the medical officers and attachés of the Brompton Hospital have not contracted phthisis. This would be the last place in the world to look for the disease as the result of contagion, for every one knows who has visited that hospital, that hygiene and regimen are most scrupulously carried out to the highest point of excellence known, the nurses and other attachés being on duty only a portion of the twenty-four hours, and when on duty are not constantly in the wards. Compare this with the manner in which patients are cared for in private practice. The nurse, a member of the family or friend of the stricken individual, generally occupies the same room, day and night—more especially in the advanced stage of the disease, and the hygiene and regimen do not, except in a few instances, receive proper attention; in some cases it is wholly neglected; every crevice about the windows, and sometimes even the key-holes, as I have more than once seen, are plugged up for fear a little fresh air might get into the room and the patient “take a cold.”

As a rule, the nursing of the phthisical in private practice is unskilled, and the circumstances under which the nurses perform their office, render them more liable to fall victims to the disease. Cases of phthisis due to contagion *have*, nevertheless, occurred at Brompton Hospital, for Walshe⁹ makes the following statements in regard to his assistants :—

“Curiously enough, of the first three clinical assistants I had at Brompton, two died of phthisis, and the third left the establishment with slight hæmoptysis, cough and chest uneasiness. The latter is now (1871), in perfect physical condition, one of the former had clearly been affected before he came to the hospital, the other was a model of sturdy health when he took the office.”

He says further :—

1. Clinical Soc. Trans. London, 1874, vol. viii, p. 144.

2. Amer. Journal of the Med. Sci., July, 1878.

3. Ibid., April, 1878.

4. Medical Record. New York, May 19, 1883.

5. Ibid.

6. Ibid.

7. Ibid, March 22, 1884.

8. Medical News. Phila., Jan 19, 1884.

9. Diseases of the Lungs, London, 1871, p. 459.

" * * * I must confess my belief in the reality of such transmissibility has of late years been strengthened. I have met with so many examples of the kind, that 'coincidence' becomes itself an explanation difficult of acceptance. I have besides, in three instances, seen a robust husband become distinctly and actively phthisical, as shown by general and local symptoms and physical signs, and on the death of his phthisical wife, whom he had closely tended, fell into the retrogressive stage of the disease, and ultimately practically recover."

" Hereditary influence in producing the disease is not as great as many believe, and all efforts have failed to prove, by statistics, the existence in a majority of the phthisical of an unfavorable tubercular family record. Walshe * says:—"The final conclusion flowing from the analysis of the family history of 446 persons is, *that phthisis in the adult hospital population of this country is to a slight amount only a disease demonstrably derived from parents.*" "Of 374 cases occurring in old women at the Salpêtrière Hospital, reported by Piorry, 78 died without presenting any traces of tubercle, although their parents died from that disease."† Dr. Cotton, who analyzed 1000 cases at Brompton Hospital, found only 365 cases in which hereditary taint could be proved; Scott Allison's observations, at the same institution, show, in 603 cases, an hereditary influence in but 19. Walshe concludes, after most careful investigation, that not over 26 per cent. can be traced to hereditary taint. How then are we to explain the cause in the remaining, we will say, 60 per cent.? Are they to be traced to pneumonias, pleurisies or kindred diseases? Or are we to conclude that there is a *specific poison* to which they may be exposed which produces this disease? I think there is, in fact there must be such a specific poison.

I will here give a hypothetical case, in order to show *the fallacy of hereditary transmission of disease*:—A. B. is stricken with tuberculosis, the family are amazed at the announcement of the fact by the attending physician, and they state that the parents and grandparents are still living and enjoying good health; the family trace back to the third generation and find that a great-grandmother died of phthisis, and this gives some satisfaction as to the disease being in the family. And if we go back three

* Diseases of the Lungs, 4th ed., London, 1871, p. 462.

† Quoted by Dr. Durant, Trans. of the N. Y. State Med. Soc., 1871, p. 172.

generations to find the cause for tuberculosis, why not go back four or five if nothing is found in the third, or failing to find it in these go back as far in the family history as tradition may extend, and finding one ancestor who *probably* died of phthisis, to conclude that therefore the case we see is transmitted from that ancestor. This, to my mind, is absurd, and yet I know the tendency of family and physician (particularly the latter) to look for this, in the transmission, rather than in an acquirement in the individual, *per se*. It is not sufficient to declare hereditary transmission when even some of the children of phthisical parents, while partaking of their delicate constitutions (that is delicate in figure and lacking ruggedness) will live to good round ages and perish from other diseases.

I do not assert that exposure to the poison will produce the disease in all individuals any more than other zymotic poisons will, for there are many who are for the time at least insusceptible to their action, and this is owing to the fact that they present no proper nidus for the poison-germ, yet from this we are not to argue that the germ itself is less potent to an individual susceptible to it. The belief in the contagiousness of the disease is as old as its literature itself. As long ago as 1668, Riveris,* who for more than twenty-five years was professor of physic in the University of Montpellier, in speaking of the causes of phthisis, wrote as follows: "Moreover, there are external causes, as contagion, which is the chiefest; for this disease is so infectious, that we may observe women to be infected by their husbands, and men by their wives, and all their children to die of the same, not only from the infection of their parent's seed, but from the company of him that was first infected. And this contagion is more easily communicated to those that are of kin, wherefore it is not safe for a brother or sister to enter into a chamber, for the *miasmata* or infective vapors, which come from their lungs and infect the whole air of the chamber, and being drawn in by others (especially if they are any way disposed to the same disease) beget the same disease in their lungs."

Not only among the members of our profession, but among all classes of people, the belief is prevalent, especially in Italy, Southern France, Spain, Portugal and Mexico. At the Canary Islands they look upon consumptives as little better than lepers,

* Practice of Physic. London, 1668, p. 170.

and they are kept in a species of quarantine, being subjected to many vexatious restrictions in regard to their intercourse with the indigenous population. This would serve to show that there must be some well-grounded reason for such belief, and it ought not be regarded as superstition.

It is impossible to comprehend how a disease, specific in its character, and definite in its course can be transmitted from parent to child; how the germ comprising the complicated organism of man could develop from the microcosm into a highly complex creature, carrying with it the elements of destruction as a part and parcel of its structure. Such teaching is opposed to all known biological facts, and it seems that writers have fallen into the fashionable professional rut in searching for the etiology of many diseases, and in none more deeply than ascribing hereditary transmission, when in reality they should say an hereditary predisposition to certain diseases.

There are a number of authorities who hold the opinion that phthisis is transmitted from parent to offspring, and among the number is Sir Wm. Jenner * who states—"That tuberculosis is transmitted from parent to child, is one of the established facts in medicine." This is absurd. If the disease is transmitted why does it remain latent for so many years? There is no such thing as the direct transmission of a tubercular virus from parent to offspring, this has been shown by such pathologists as Guizot, who, "in four hundred post-mortem examinations of the bodies of new-born infants, failed to find a single deposit of tubercle, and Gluge asserts that there is no born tubercle."† Tuberculosis to-day is the same, and manifests itself in the same manner that it did centuries ago. It reveals the same pathological appearances in one case as in another, and maintains its specific character under all circumstances. How then is it possible to harmonize known facts with the doctrine of hereditary transmission, when diseased parents and the east wind are equally effective in producing the same specific result? That constitutional peculiarities are not pathological, needs no argument; and therefore our faith in their transmission need not be put upon the stretch in acquiescing in this belief. Nor is it to be denied that constitutional peculiarity may be acquired and still leave the body in a physiological con-

* *The Practice of Medicine To-Day.* London, 1869, p. 48.

† Quoted by Durant, *Trans. of the New York State Med. Soc.*, 1878, p. 174.

dition. As an instance: Most persons have a transmitted constitutional condition of body that may be infected by the virus of small-pox; this habit of body may be so altered, by vaccination or otherwise, that it cannot be infected, and still leave the body in a physiological condition. The susceptibility was transmitted and it is destroyed. On the other hand, the susceptibility may be acquired instead of being transmitted, so that he who was born constitutionally protected may become jeopardized, if exposed to the infecting influence of the small-pox germ. Even in the propagation of this highly infectious disease two distinct factors are engaged, between which there is a perfect coadaptability, if I may so speak, for, when they are brought together they combine to bring about a certain result, uniform in appearance and constant in character.

We have a marked illustration of the fact that certain modified conditions of the parents are not transmitted to the offspring, in the case where parents have been vaccinated and yet there is no protection from small-pox in the child. This has been aptly compared to the soil and the seed. The earth could not bring forth fruit without the seed, the seed could not germinate and reproduce itself without the soil. The soil in order to be productive should be fertile, and the seed should embody within itself the elements of life. The condition of the body, like the condition of the soil, determine its efficacy as a factor and its relevancy to a specific result, when joined to that other factor, the seed or germ, in the production of disease. The body is the soil for disease-producing germs. If the body does not offer a suitable nidus, the seed planted therein could not germinate, grow, bud, bloom or fructify. This peculiar condition of various parts of the animal body, which offers a suitable soil to disease-producing germs is familiarly known in medicine as *predisposition*. It is that which is transmitted from parent to child—the predisposition to certain diseases, and not the disease itself. A tuberculous parent may transmit this soil, this habit of body, this *predisposition*, to his or her offspring, but cannot under any circumstances at the same time transmit the seed in a dormant state already planted in that soil. Dissections, as already stated, have not revealed tubercles in the new-born. They may be born with many physical imperfections but never with any trace of tuberculosis. The individual must be subjected to disturbing

extrinsic causes before there are any evidences of tuberculosis, and when such manifestations do occur, they are of a peculiar and constant kind. One case of tuberculosis is as much like another as one case of small-pox is like another of that disease. It would indeed be a strange coincidence, and one that could not be accounted for, by any telluric or atmospheric influences so variable in their nature and uncertain in their operation. When we observe a constant recurrence of symptoms and pathological changes in a series of cases, we naturally conclude that a specific cause is operating upon a peculiar condition of body to produce such a uniform result. It is evident that the offspring of phthisical parents sometimes escape the disease for M. Pidoux states that:—"Not over twenty-five per cent of those born of consumptive parents themselves become phthisical."*

The predisposition is not only inherited, but is also acquired by the offspring of healthy parents; thus parents of non-phthisical children may themselves acquire the disease under conditions favorable to its development. It is not contended by those who believe in and know the fact of the contagiousness of phthisis, that the disease is thus contracted as frequently as other infectious or contagious diseases are acquired; but, I am free to say, however, that there is far more danger to be dreaded from nursing the phthisical in private practice, than there is from nursing cases of typhoid fever. In the latter disease, the "*materies morbi*" reside in the excreta, and by cleanliness the infectious element is promptly removed and the danger lessened; this is not the case in phthisis, for in that disease the "*material cause*" resides in the effete matter constantly being thrown off from the lungs of the stricken individual, especially in the advanced stage of the disease. This has been proved by Ransom,† who found the bacillus tuberculosis in the air of a room containing several advanced cases of phthisis. Dr. R. Charnley Smith,‡ detected them in a respirator worn by a phthisical patient, and Dr. C. T. Williams,§ by an ingenious method, has found the bacillus in fair abundance in the extracting flues at Brompton Hospital. The tubercular bacillus is characteristic, and can readily be discriminated from all other bacilli. It has been found in all the tubercular lesions of the organs and

* Quoted by Dr. Durant, Trans. of the N. Y. State Med. Soc., 1871, p. 172.

† A His. of Tuberculosis by E. E. Sattler. Cin., 1883, p. 164.

‡ The Lancet, Jan. 20, 1883.

§ Ibid., July 23, 1883.

tissues of the body of the phthisical, including, of course, the osseous system and its medullary substance. It has also been found in all the secretions and excretions of organs similarly affected. It is a well-known fact that phthisis prevails to a great extent in most of the European armies, and this prevalence can only be accounted for by the contagious character of the disease. As an evidence of this, it might be stated that Surgeon General von Lauer,* of the Prussian Army, has recently issued a circular to the medical officers, directing them not only to isolate the phthisical from the non-phthisical, but *that special means be taken for the disinfection of the sputa in tuberculous cases.*

"Sir Wm. Wild,† in the Irish Census Reports for 1851, states that in the years 1847, '48, '49, there died of phthisis, in Ireland, 66,000 persons, or 22,000 per year." This occurred before the tide of emigration to this country set in. So frightful a mortality can only be attributed to the crowding together of people obliged to breathe an atmosphere loaded with the germs of this disease.

It is well known that the red, as well as the white blood-corpuscles sometimes leave the vessels and migrate through the adjacent tissues. They have even been seen to enter the vessels. These corpuscles, as is also well known, are from the $\frac{1}{3200}$ to the $\frac{1}{4000}$ of an inch in diameter. How much easier would it be for the *tubercle bacillus*, which is generally described as varying in length from the $\frac{1}{32000}$ to the $\frac{1}{4000}$ of an inch, and having a breadth of $\frac{1}{8}$ of its length, to escape from or enter the blood-vessels? It is through the medium of the lymph and the blood that these bacilli are carried throughout the system. The tubercle is developed according to, and is governed by its own laws, as are the eruptions of small-pox, or the formation of false membrane in diphtheria, and who will deny that there is a *specific contagium* in the blood, in cases of these diseases, before such local manifestations occur? I hold that tubercle, as we know it, is never a primary product; it derives its origin from the action of a pre-existing *materies morbi*. The lymph or the blood carries the morbid material throughout the body, and certain organs attempt to eliminate the poison, but failing to do so become themselves the ground in which the poison accumulates. Why certain organs

* Sanitary Engineer, 1883, vol. viii, No. 20.

† Dublin, 1856, vol. i, p. 447, also quoted by H. McCormac, M. D., "On Consumption," London, 1866, p. 225.

are more prone than others to become the receptacle of these deposits, I will not here attempt to explain. Perhaps the peculiar structure of the lungs, and the fact that all of the blood of the body must and does pass through them in great quantity and with great velocity, may be one reason why they are apt to be the seat of tubercular deposit, the softening and breaking down of which is the result of an inherent irritation of the *materies morbi*; this being a foreign matter the lungs rebel against its presence and make an effort to cast it off. More or less perfect parallels are seen in the exanthems, where the skin becomes the eliminating organ. In diphtheria, where elimination is by the mucous membrane, and in small-pox, scarlet fever, measles and erysipelas, diseases each having its own peculiar and characteristic eruption, each respective disease being due to the presence and action of a peculiar characteristic *materies morbi* prior to the appearance of local symptoms. In the attempt at elimination or resistance to the invasion of these poisons we have the lesions so characteristic of them and generally recognized as belonging to the disease. Do they constitute the disease, comprising both its cause and effect? By no means; they are but the expression or effect of the cause, precisely as tubercle in the lung or elsewhere is but the expression or effect of a pre-existing *materies morbi*.

The germs producing these diseases are not diseased germs; they are germs in perfect health as to themselves and are capable of producing disease only where a nidus suitable for their development is found. It seems to me unnecessary to enter into any labored course of reasoning or offer any lengthened recital of examples, further than has been done in this paper. There is still a ring of mystery in the minds of some physicians about this whole matter. But when, in the near future, the smoke and dust have been made to subside by the great workers who are now engaged in this promising field of labor, and when all the avenues and by-ways are macadamized, so to speak, by their results, then will this much mooted and intricate subject be far on the way to a final settlement, and millions of lives saved annually from premature graves.

So thoroughly am I impressed with the importance of this question, and of the incalculable advantage a thorough understanding of its bearings must be to the members of my profession,

that I feel I have not consumed time unprofitably in my effort, however faulty it may be, to set before you the facts which have convinced me of the contagiousness of a so fearfully common and invariably fatal disease as tubercular phthisis.

AMPUTATION OF LEFT LEG AND THIRD, FOURTH AND FIFTH METATARSALS OF RIGHT FOOT.

BY WILLIAM S. JANNEY, M. D.

Read April 16, 1884.

MR. Clarkson, a colored man, æt. 41, was admitted to the Surgical Department of Philadelphia Hospital, December 26, 1883. He was a driver for the Messrs. Bumm, salt merchants of this city. He was exposed to the intense cold of the third week of last December. A few days before his entrance to the Hospital, he noticed that his feet were swollen; he cut his shoes and continued to work until the 22d. On the 26th, both feet were swollen, the left the most, with the characteristic appearances of gangrene. In a few days sloughing commenced, and charcoal poultices were applied. On the 9th of January the line of demarcation on the left ankle was evident. On the 16th of January the line was deeply cut, and had begun to form on the right foot from the second interdigital web backward to the base of the fifth metatarsal. On the 16th, the left leg was amputated four inches above the ankle-joint, by a double tegumentary flap operation.

The stump was dressed with carbolized oil, and syringed out with carbolized water, 1-40 daily. The patient's general condition was bad.

At the end of the fifth day the wound was dry, and gangrene had destroyed half the upper and nether flaps. The mortified portion was cut off with scissors, the edges of the flaps were approximated with wire sutures and straps of rubber adhesive plaster, which the intentional redundancy of the flaps permitted, the wound healing without accident.

The tissues of right foot continued to slough. On the 6th of February it presented the following appearance: The third, fourth and fifth metatarsal bones and their phalanges were completely denuded, except the tendons of the extensor muscles, which remained attached to their respective insertions.

In consultation with my colleagues, it was thought that no operation short of Chopart's would be likely to succeed, as the remaining integument and underlying tissues covering the first and second metatarsal and their phalanges were infiltrated and boggy, indicating a very low vitality.

Mr. Clarkson was very desirous of having as much of his foot saved as possible, and with his approval I disarticulated the third, fourth and fifth metatarsal from the tarsal bones, leaving an open wound, which was dressed the first twenty-four hours with carbolized oil. Gangrene speedily appeared in the wound; a poultice of tar, iodine and flaxseed was crowded between the gaping edges; in three days the wound was clean and full of granulations; the wound was then dressed with a solution of bichloride of mercury, 1-1000; straps of adhesive plaster were applied, so that the edges were gradually approximated. At present the left stump has healed entirely, except a slight break, through which a necrosed portion of the bone was removed; the right foot has healed; the foot is in the condition of slight varus, but is very serviceable; the man's general condition is good.

The internal treatment was quinine, iron and chlorine water.

TWO CASES OF INTESTINAL OBSTRUCTION: ONE FROM IMPACTED FÆCES; THE OTHER BY STRICTURE DUE TO CANCER, WITH SPECIMEN.

Read June 18, 1884.

BY DR. GEORGE W. VOGLER.

MODERN writers usually enumerate the causes of intestinal obstruction as follows:—1. Congenital malformation. 2. Internal strangulation. 3. Intussusception or Invagination. 4. Constriction. 5. Compression. 6. Impaction of foreign bodies or intestinal concretions. 7. Impaction of fecal masses.

Upon looking through the statistics of intestinal obstructive diseases, certain striking facts are noticeable. It is difficult to estimate the comparative frequency of intestinal obstruction. It is by no means a very common affection, always dangerous, and in a very large proportion of cases resulting fatally. Statistics show that most forms of obstruction of the bowels are more often met with in the male than in the female subject, the exceptions

being obstruction by gallstone or fecal matter; constrictions by peritoneal or other adhesions, and compression of the intestine by tumors or displaced viscera.

As regards age and the portion of the intestine involved:

Obstruction by gallstones always occurs late in life, and involves the jejunum and ileum. Intussusception may occur at all ages, especially during childhood, and is confined chiefly to the large intestine.

Stricture is a disease of adult life (of course always omitting congenital malformation), involving the large intestine; three-fourths of the total number being below the middle of the transverse colon.

With these few introductory remarks I will proceed at once to the reading of the notes of a case of intestinal obstruction by constriction due to cancer:—

Mrs. Mary Müller, German, aged 58 years, widow, was admitted April 18th, into the ward of my friend and colleague, Dr. Adam Trau, at the German Hospital.

For some six months previous to her admission she had been under almost constant treatment by physicians. She states that constipation was her chief symptom, for which all kinds of purgative remedies and various procedures were used, with but little avail.

She was anæmic, considerably emaciated, and very weak. Chief symptoms were obstinate constipation, great tympanitis, severe pain, and almost incessant vomiting. Temperature and pulse not much disturbed.

A careful physical exploration of the abdomen, vagina, and rectum, elicited no sign of a tumor.

A long œsophageal tube was readily passed into the bowel, even to the length of twenty-seven (27) inches, according to the statement of the medical resident.

In brief, it seemed that the case was one of a paresis of intestinal movement, due to some defect either in the intrinsic ganglia and nerves of the muscular intestinal coat, or through imperfection of the muscular tissue by degeneration; or, both these causes combined.

The treatment consisted in the use of purgative remedies, large watery and stimulating enemata, with comparatively no results; turpentine stupes, followed by hot flaxseed poultices gave some ease. The introduction of a long œsophageal tube into the bowel and massage of the abdomen afforded much relief. So great was the tympanitis shortly before her death that respiration was materially interfered with, necessitating the introduction of a small aspirating needle to afford temporary relief.

She was fed by pancreaticized milk per bowel, and Cibil's beef extract and stimulants by mouth.

Death occurred April 29th, by asthenia.

Owing to the interest taken in the case during life, the following gentlemen were present at the post-mortem examination, by invitation of Dr. Trau: Drs. Formad, Dercum, F. H. Gross, Barton, Jones, Vogler, and the residents, Weed, Stabler and Rehfuss.

Body emaciated and abdomen greatly distended, the intestinal convolutions plainly mapped out upon the abdominal wall. The distension of the intestines, particularly the colon, was simply enormous. Some adhesions noted. At the lower part of the descending colon, a constriction was seen. This part of the intestine was carefully removed, and when subjected to the hydrostatic test, was found to be impervious to a downward current, but readily permitted the passage of an upward current. On opening the specimen along the line of the mesenteric attachment, a valve-like scirrhus tumor was exposed, which readily accounted for the phenomena observed in life, viz.:—1. Ready introduction of fluids into the bowel by a fountain syringe, also a rectal tube. 2. Failure of the fluid to be voided after withdrawal of the tube. 3. An almost total absence of the natural passages from the bowel.

The specimen which I now pass around is well worth a careful inspection, the lesion consisting of two valve-like scirrhus masses completely encircling the bowel, to which it is alone confined.

The other case of intestinal obstruction was due to impacted feces:

Mrs. F., aged 64, private patient, afflicted for many years with habitual constipation, frequently going many days without an evacuation; commenced complaining some three weeks previous to my visit, with severe pain in the left lumbar and umbilical regions, and an impossibility to relieve the bowels by powerful purgatives.

No other symptom of note troubled her up to within a few days, but now she was suffering with some fever, pain, nausea, exhaustion, and was passing some bloody mucus from the bowels.

Careful examination of abdomen and vagina, revealed a large, hard and irregular-shaped body or tumor, situated in the left lumbar region, painful to the touch, slightly movable, and entirely free from womb and its appendages.

The diagnosis was determined by the history, the absence of acute symptoms, physical examination, and by the process of exclusion of other causes of obstruction. By this time her life was in great danger.

The treatment consisted chiefly in the frequent use of large enemas of soap-water, castor oil, turpentine and laudanum.

Small doses of calomel and ipecac were given by mouth. After two or three days, some small and hard pieces of fecal matter were passed for the first time, and a careful examination indicated the mass to have broken in two, one occupying the old position; the other, the sigmoid flexure.

She was vomiting at this time.

The quantity of fecal matter that came away during the following week was enormous. At one sitting, as many as twenty-seven large pieces of hard, dry fecal matter came away, and I give this merely as an illustration of the quantity passed at one time. Frequently it became necessary to unload the rectum by digging out the masses.

At times bloody mucus came away.

Of course, pain was very great during these operations, and opium had to be resorted to.

Gradually the swellings began to grow less, and finally disappeared. The patient was able to trace their slow movements along the intestines by the intense pain.

Only after weeks of careful treatment did this patient fully recover her health, and, strange to say, with a radical cure of her habitual and long-standing constipation.

THE DEMAND FOR EARLY EXPLORATORY TREPHINING IN DEPRESSED FRACTURES OF THE SKULL.

Read June 18, 1884.

BY JOHN B. ROBERTS, M. D.,

Professor of Anatomy and Surgery in the Philadelphia Polyclinic.

IN using the term trephining I apply it to all methods of removing portions of the cranial wall, whether by the trephine, saw, burr of the surgical engine, gouge or cutting forceps.

From observation, experience and considerable acquaintance with the literature of the subject, I am convinced that surgeons are induced to decline or postpone the operation of trephining because of a mistaken idea of its serious nature, and a misunderstanding of the reasons for its adoption.

The frequency with which successful trephining was done in past centuries without the benefit of our improved methods and instruments, and the infrequency of death or serious symptoms from the operation itself at the present day, convince me that, though a capital operation, it is not one having in it many elements of danger. That many deaths occur after trephining is admitted. Such fatal results must often occur, for injury to the skull bones is usually and almost necessarily coincident with disturbance or actual lesion of the brain or its membranes. I

believe that more deaths are attributable to non-performance or delay in resorting to the performance of trephining than to its adoption. It behooves those who greatly limit the application of the operation to prove, by citation of cases, that a fatal issue has been induced by the procedure itself in a sufficient number of instances to throw the operation into the class called dangerous. To merely show that many cases of serious skull injury recover without trephining is not sufficient.

Much controversy on this subject would be avoided if the advocates and the opponents of an extended use of trephining would clearly formulate their opinions as to the theory upon which the operation is performed. In my opinion, trephining should be regarded in the light of an exploratory rather than a therapeutic procedure. I incise the scalp, in closed fractures of the skull, not because the incision cures, but because it tells me the condition of the bone, without which knowledge I am unable to treat the patient rationally. The uncertainty of the lesion is, in my opinion, more dangerous to health and life than the conversion of a closed into an open fracture of the skull, because the observation of the profession teaches that open cranial fractures do not resemble, in fatality, similar open fractures of long bones. In truth, I would be willing to make a closed fracture of the thigh or leg an open one, if it was otherwise impossible to replace fragments which were threatening life. If I but learn the character of the skull lesion, I am acquainted with surgical expedients that render restoration to health more probable than the complication due to the incision renders it improbable. Hence I am justified, nay, compelled, by my reason, to advocate exploratory incisions of the scalp in obscure injuries of the skull.

The same line of reasoning forces upon me the conclusion that I should trephine whenever the fracture, whether originally an open one or so made by my incision, presents the possibility of the inner table being detached and splintered more extensively than the outer. In other words, I should cut the scalp to see the condition of the outer table; I should cut the bone to see the condition of the inner table, in every case where the risk of obscure knowledge is greater than the risk of divided scalp and perforated bone.

Many experimental fractures made in the dissecting-room, and observation of cases in the practice of myself and of others, teach me that extensive shattering of the inner table, with only a

moderate amount of fracturing of the external table, is of frequent occurrence in other as well as in punctured fractures. I admit that the condition in the dead subject, with its shrunk brain, is different from that in the living; but there is much evidence of the same splintering to be found in the study of accidental and homicidal skull fractures. Punctured fractures have long been treated by early trephining, to avert encephalitis. For the same reason I recommend resort to trephining in more diffused and less accentuated fractures. It is to prevent inflammatory sequences due to splinters forced into the membranes and brain, and to avert the consecutive occurrence of epilepsy and insanity, that the operation should be performed; not because of the fear that symptoms of compression of the brain may arise, nor because necrosis of detached portions of bone may occur.

I am not a believer in the pathology that teaches that the symptoms which we call "compression of the brain" are due to displacing pressure exerted on the brain substance. How can a slight or even a considerable depression of a limited area of bone produce much pressure upon the brain substance? How can the usually limited extravasation of blood under the seat of fracture fatally compress the brain, which is of firmer consistence than the blood itself? A rapidly acting heart, after violent exercise, will throw enough additional blood into the cerebral vessels to produce more intracranial pressure than the ordinary depressed fracture. The complexus of symptoms called compression of the brain may possibly be the result of a disturbance in the local capillary circulation of the membranes and subjacent nervous tissue; but I cannot believe it to be due to compression or displacement of the brain itself. It is more probable that compression symptoms are the results of encephalitis, due to injury from spicules of the inner table, or to the irritation of intracranial bleeding.

As soon as the profession repudiates the idea that brain displacement is what causes compression symptoms, so soon will every surgeon be convinced that early trephining is a proper exploratory procedure in order to determine what measures are demanded to avert encephalic inflammation.

"Compression of the brain," as seen after injury, should be translated "inflammation of the brain," and looked upon as probably due to unrelieved irritation of the brain periphery, from traumatic causes. Not until this is so understood will the dis-

cussion as to the utility of trephining in depressed fractures cease.

I repeat, then, that trephining is not a therapeutic but an exploratory operation; and, as such, is demanded with much greater frequency than is usually supposed. If it is to be employed for exploratory and diagnostic purposes, early resort thereto needs no defense.

When about to use the trephine itself for perforating the skull, to allow elevation and extraction of fragments, the surgeon should select a small conical instrument; one not over three-eighths of an inch in outside diameter at the cutting end is large enough. Those usually kept by the instrument makers are too large. It is only necessary to bore an opening sufficiently large to admit the end of the elevator; hence a small trephine is always more proper than a large one, except in those comparatively rare cases where a large disk is to be removed *over* the line of an old depressed fracture. Recently, I visited the four principal instrument makers of Philadelphia, and could not find in stock any trephine as small as that which I recommend. The belief which has caused trephines to be made so large is founded on an erroneous theory.

In recent depressed fractures the trephine crown should be applied upon the solid bone, and should overlap the *least* depressed edge of the displaced fragment. This allows more ready elevation or extraction by means of the elevator, because the *most* depressed edge is very frequently beveled, with the inner table broken at a more distant spot, and is thereby wedged under the solid portion of the skull at that side. Elevation at the least depressed edge is effected more readily and with less danger to the brain from the manipulation.

To conclude, I assert that in all subcutaneous injuries of the head with possible existence of depressed fracture, an immediate exploratory incision should be made in the scalp. In all instances of depressed fracture with *possible* existence of splintering and spiculation of the inner table, an immediate exploratory trephining of the skull should be done.

DISCUSSION ON TREPHINING.

DR. NANCREDE: I have listened with much interest to Dr. Roberts' paper, which I consider an opportune one, in view of the dread in the minds

of the public and of the profession of the operation of trephining *per se*. Whatever might be the *practice* of leading surgeons, their teaching was to discourage the use of this instrument. If we examine the mortality attendant upon operations performed for epilepsy, pain in the head, etc., in a large number of cases collected by Walsham, Briggs and myself, we shall find that only about 10·89 per cent. prove fatal. Should we subtract from these certain cases where fatal complications resulted which did not inhere to the operation itself, the mortality would be still lower. Even leaving it at 10·89 per cent., we perform other operations more fatal in themselves to relieve less dangerous *possible* results than follow depressed skull fractures. There are three varieties of encephalitis from head injuries, the first of which results from ultracranial damage, caused by the force of a blow, and which would result in intracranial inflammation, whether the skull be broken or not; a second, which is the result of the injury to the encephalon from its wounding by depressed, lacerating bone fragments; and a third, at first neither fatal injury to the brain from the mere shock of the blow, nor from depressed fragments. In this latter class, the immediate removal of irritating spicules, against which the brain and its membranes must be driven from seventy times per minute upwards, by the pulsations of the heart, would in a majority of cases suffice to avert encephalitis. In the other two classes, trephining would indeed remove all removable sources of irritation and future causes of inflammation, but as it could not restore a damaged brain, the operation too often failed to avert a fatal issue. It does not *add* to the risk in such cases, but on the other hand much diminishes it especially with antiseptic precautions. It was the custom to call such cases "fatal trephinings." This was all wrong, the lethal issue being the result of the primary injuries and not of the operation. Early operations in head injuries are more favorable than late; primary trephinings giving about 22 per cent. of mortality against nearly 53 per cent. for secondary operations. These are from data from my own statistics.

I wish to point out the unreliability of the statistics of Fritze, Bluhm, Pirigoff, etc., as demonstrated by the researches of Walsham, which show that these tables contain cases where no operation was performed, or where death occurred from a ruptured liver, or other serious visceral injury. A large proportion of deaths after trephining, when their histories are carefully scrutinized, show that the operation had nothing whatever to do with it. I repeat, that the paper is a timely one, and, while not going all lengths with Dr. Roberts, I would certainly cut down upon a simple depressed fracture without symptoms of compression, *provided the bony depression was marked and irregular*, to lessen the chances of encephalitis and epilepsy. This is the advice given by the elder Gross, by Prof. Moses Gunn, and is, I believe, the practice of the surgeons of the Penna. Hospital. A discussion held in this room, only three years since—I refer to the Am. Surgical Association's debate on Dr. Gunn's paper—proves the assertion, that whatever the *practice* of surgeons might be, their teachings are as a rule distinctly against the use of the trephine. I for one always trephine for compound comminuted depressed fractures, whether there were compression symp-

toms or not. Dr. Roberts, being absent, has evidently allowed a clerical error to remain in his paper, viz.: that compression symptoms meant inflammatory symptoms, his evident meaning being that when symptoms of *secondary* compression supervened that they were the result of the pressure of inflammatory effusions.

DR. J. M. BARTON: The cry urging more frequent use of the trephine in head injuries, is an old one. In all the various editions of the various *Surgeries* published in the last twenty years, many cases are cited to show how little risk attended the operation, many individuals who had been trephined eight and ten times, are mentioned, and all wind up with the famous old case of the Count of Nassau, who had been trephined twenty-seven times.

I have no doubt that more patients are lost by the trephine being withheld than by its too frequent use, but I am not yet prepared to go as far as the lecturer of the evening, and use it merely as a means of exploration; even the strongest advocates of the operation admit a death-rate of ten per cent. from the operation alone; this is a risk we have no right to take unless the symptoms warranted it.

DR. LEVIS: I feel sure that the points advanced by Dr. Roberts are not in accordance with general teaching on the subject, but are in accord with the modern practice of surgery. The method of trephining at the least depressed portion of the fracture, permits the use of a much smaller trephine, and is much more satisfactory than the old method of trephining over the most depressed portion. Indeed, as has been said by Dr. Roberts, it is not possible to obtain in the market an instrument as small as is suitable for use. My own practice in the Pennsylvania Hospital and elsewhere has been similar to that advocated in the paper. I relate a case of depressed fracture, in which I neglected to perform the operation, and had the mortification of seeing the patient become an epileptic, a result which might have been prevented by the use of the trephine.

(Dr. Levis illustrated, by sketches on the blackboard, the condition of the skull in ordinary depressed fractures and the manner of applying the trephine.)

DR. DAVIS: Dr. Roberts has omitted to make any reference in his paper to the application of the antiseptic method of treatment in cases of trephining. This I believe to be an important point, as I think it lessens the dangers of the operation considerably, particularly in those cases where a simple fracture is converted into a compound one by the surgeon himself. Putrefaction is just as liable to occur in cases of trephining as in other operations, and the same precautions should be taken to guard against it. I have seen it occur in one case in a very marked form. A man was admitted to the Pennsylvania Hospital with a wound of the scalp. It had already been dressed. I had his hair cut short and examined him, but discovered no fracture. His physician accompanied him and said he had found none. The man was feeling badly, so I took him into the hospital. He was seen by the visiting surgeon, who detected no fracture, and who allowed

the wound to remain closed. On the second or third day, brain symptoms set in, but not in such a marked form as to induce the surgeon to reopen the wound. The next day he was worse and had a chill, so I called one of the other surgeons of the staff to see him and he ordered the wound to be reopened and the skull trephined if a fracture was found. I opened the wound and found a fracture one and a quarter ($1\frac{1}{4}$) inches long, semilunar in form, with a depression of about one-sixteenth ($\frac{1}{16}$) of an inch. On trephining, the inner table was found to be splintered, one of the pieces having punctured the dura mater. Through this puncture, pus, brain substance, and serum, mixed, oozed. I enlarged the opening, leaving out a large amount of liquid. I washed the cavity out with a weak carbolic solution and left the wound open. The evacuated material was in a state of putrefaction, its bad odor being very marked. The man improved for a day after the operation, but then got worse and died soon after. This case illustrates two points: first, the difficulty of arriving at a correct decision as to when to interfere in cases in which brain symptoms are coming on, and second, that if the causes of putrefaction could get through that tightly-wedged fracture and cause putrefaction within the skull, how much more liable is it to occur in the large wound left by the incision, both in the soft parts and bone, which the operation of trephining would make. Therefore, in cases in which trephining is done as a preventive measure particularly, I think the antiseptic method should be practiced. I cannot agree with Dr. Roberts' proposition, to trephine at the point of lesser instead of greatest depression, nor with Dr. Levis's remarks in support of the same. Such cases as the latter describes could only be produced by a blow from some such instrument as a hammer, which punches out a small disk of bone. On the other hand, according to my observation, the most usual form of compound fractures of the skull are those in which, while one end of the fragment is depressed, the other still remains attached to the uninjured skull, and the bone being trephined at the point of greatest depression, the depressed portion is pushed backwards and upwards into place with the elevator. The advantages of this mode of operating are obvious. The piece of depressed bone acts as a lever, the fixed point or fulcrum being at its point of attachment to the sound skull, and the force being applied at its movable or depressed end. By this means the greatest power is brought to bear at the most advantageous point. Any one who has seen such operations knows that sometimes considerable force is necessary to restore the depressed bone, and if the elevator is applied at any but the most depressed portion, power is lost just in the proportion as its point of application recedes from the point of greatest depression and approaches that of its attachment. The reason why considerable force is necessary sometimes, is that one must overcome the impaction of the fragment at its point of attachment and at its sides. The impaction at its sides steadily diminishes from the point of its attachment to the point of greatest depression. The fact of the free end of the depressed bone underlapping the edge of sound bone does not influence the truth of the proposition at all.

DR. WM. T. TAYLOR: I remember that within the last five years I have read in the *American Journal of the Medical Sciences* several articles, by Dr. Liddell, on injuries of the skull and brain, in which is advocated early trephining as being requisite for the treatment of brain lesions; his views being similar to those presented by the author of the paper.

ETHERIZATION BY THE RECTUM; REPORT OF FOUR CASES BY YVERSEN'S METHOD.

Read June 18, 1884.

BY JOHN S. MILLER, M. D.

I DESIRE to report four cases of etherization by the rectum, a method of producing anæsthesia first suggested by Dr. Axel Yversen of Copenhagen.

These cases were in my recent practice; and to Drs. Louis Jurist and A. B. Hirsh, I am indebted for assistance rendered, and for many of the observations made. In two of these cases the mucous membrane of the bowel was prepared for its respiratory function, as it ought to have been in all, by a restriction of diet and the use of purgatives. No preliminary hypodermics were used. The method of administering the ether was simple. A definite quantity was placed in a bottle (only partially filling it), was vaporized by a water-bath at 120°, and the vapor conducted to the rectum by a rubber tube, terminating in a recurrent catheter, the free or recurrent end being closed by pressure of the thumb during the inflation of the bowel; the expiratory act was performed by removing this pressure, and removing the water-bath.

The first case was one for minor operation, demanding only primary anæsthesia. This patient had not been prepared, and sufficient precaution was not taken against the introduction of ether vapor in too great a quantity, and of liquid ether, by an overboiling in the apparatus. Almost immediately he complained of burning and tenesmus, the abdomen became promptly and greatly distended, and there were colicky pains. In about one minute he noted the taste of ether. A portion of the vapor was allowed to escape, and no more was given. The pain ceased, intoxication soon began, and in six minutes he was sufficiently anæsthetized for operation. The pulse was full, and respiration was easy. Two minutes later he returned to consciousness, but

seemed dazed. The struggling had been trifling. There was no vomiting, and no diarrhœa followed. One ounce of ether was used.

The second patient was an adult male, from whom I removed an exostosis of the vomer—an operation requiring full anæsthesia. In this case a sufficient laxative had been given the previous night. Two hours before the operation he had been allowed an ordinary breakfast. This patient, too, experienced a prompt burning and discomfort in the rectum, but at no time great, and soon ceasing. Ether was tasted in about two minutes, and noted on the breath. The abdomen seemed distended and some cramp-like pains were experienced. A considerable amount of vapor was then allowed to escape—with instant relief. After waiting two minutes without the development of further phenomena, a somewhat less amount of vapor was introduced, and (the catheter being withdrawn) was left for gradual absorption. The stage of excitement was short, marked by a pleasant delirium, and without motor activity. Full anæsthesia was obtained in eleven minutes from the first introduction of the ether vapor, and was perfectly maintained during the eight minutes of operation. Escape of the residual vapor was secured by a gentle kneading of the abdomen, and separation of the nates. The posterior nares not having been plugged, considerable blood regurgitated from the stomach after operation. This vomiting cannot, with any certainty, be attributed to the ether. No diarrhœa followed. An ounce and a half of the anæsthetic was used.

The third patient, also an adult, robust male, was subjected to acupressure of the internal saphenous vein, with destruction by means of Vienna paste of several neighboring vessels—an operation also requiring full anæsthesia. He had received a laxative the day before, and an enema on the morning of operation, and had taken a moderate breakfast. The sensation of warmth and tenesmus was immediate, but soon ceased. The abdomen became distended, and he complained of epigastric pain. A partial escape of vapor was permitted, and he had instant relief. A few minutes later the bowel was again inflated, and the tube withdrawn. Enough vapor remained after withdrawing the tube, to produce complete anæsthesia in a total of fifteen minutes; and no further introduction was required to maintain it. There had been almost no stage of excitation, and that with no other phenomena than an

immoderate laughing. He recovered promptly. No vomiting, or diarrhœa, followed. A little less than two ounces of ether were used.

The fourth case was that of a medical gentleman in good health, whose love of science led him to volunteer a passive part in these experiments. This time the bowel had not been prepared, although an ordinary movement had taken place five hours previous. On introducing the vapor, there was slight burning and tenesmus, but no cramps. Intoxication was soon induced, and the doctor seemed most of all to enjoy the proceedings. Pulse and respiration were normal. A lively peristalsis now put an end to this mode of administration, and terminated the experiment.

The only reason for quoting this case, is the evidence it furnishes for the necessity of preparing the bowel—a necessity which excludes this method of etherization from our resources in accident and emergency cases.

This case completes the four, and I have had no other opportunities for observation.

Some question having arisen, as to whether the vapor really does pass the ileo-cæcal valve, I deemed this a subject for legitimate vivisection; and etherizing a cat per rectum, opened the abdominal cavity, and noted that the small intestine was as greatly distended as the large.

In this method of etherization the most obvious advantages are as follows:—

1. Dyspnœa is avoided, and the patient is saved from the anxiety due to a sense of impending suffocation.
2. There is avoided the danger of simultaneous irritation of the superior laryngeal and pneumogastric nerves at the periphery—these irritations neutralizing each other in the respiratory centre, and suspending respiration entirely.
3. The danger of asphyxia is lessened—the patient not being drowned in his own mucus, and the integrity of the pulmonary mucous membrane as an organ of gas exchange, is preserved. Of course some vapor finds itself in the lungs, and acts there as a local irritant—elimination being by that channel. But the quantity is not great, and does not constitute a source of danger. In the cases reported, the increase in secretion was too trifling for discovery.

4. The stage of excitation is therefore not prolonged by the struggles for breath. In general it may be said that the delirium of any alcoholic intoxication is a pleasant and good-natured one, unless the patient is crossed—as he certainly feels himself to be when a wet towel is pressed over his face.

5. Nourishment may be taken before operation to sustain the powers of life, and lessen the dangers from shock.

6. Return to consciousness is prompt—this stage not being prolonged by carbonic-acid poisoning.

7. The anæsthetic seems as readily suspended as by the ordinary method—the bowel being promptly emptied by gentle massage.

8. Economy in ether is an advantage hardly to be mentioned with more important considerations.

The more obvious disadvantages are:—

1. The exposure of person required—the abdomen being necessarily under observation, even if the catheter be inserted under cover.

2. More judgment and experience is required in the administration, than by the ordinary method—over-boiling in the apparatus, and too much distension, being both painful and highly dangerous. The warning to cease is sudden, and must be immediately obeyed.

3. Just as the other mode is inconvenient in oral surgery, so in perineal operations is the apparatus needed for this method, in the way.

4. In abdominal surgery, or if there be marked intestinal lesion, this mode is contra-indicated.

5. The inapplicability in cases of accident and emergency, when time cannot be allowed to prepare the bowel, has already been mentioned.

6. Diarrhœa has been noted in seven out of the thirty-seven cases on record, though in none of mine.

I believe this sequel is due to pre-existing intestinal lesion, to the lack of preparation, to a too great distension of the bowel, or to the accidental introduction of ether in liquid form. Furthermore, my method has differed from that of other experimenters in this respect, that instead of allowing the vapor to remain indefinitely, I secured a constant change by using a recurrent

catheter, and introducing a certain quantity, or permitting it to escape, as indicated.

Other points of advantage and disadvantage may occur in later experience, and to other observers, and new dangers may be discovered. But I am convinced that this method is worthy of further trial, and will find its place in surgery, fulfilling its own, though not *all*, indications. Like all else in therapeutics it must pass through the stages of bungling use, condemnation, and revival.



C. LENTZ, Phila.

Dr. Miller exhibited a form of apparatus which he had had made by Chas. Lentz & Sons, No. 27 South Tenth Street, for this purpose. It consists simply of a water-bath, a graduated bottle provided with a funnel and valve for pouring in the ether, and a supply-pipe for conducting the vapor to the rectum. This tube terminates in a straight recurrent catheter, the exhaust channel of which is controlled by a valve. The catheter is furthermore provided with a movable collar for pressure against the anus—it having been found that the vapor tends to escape by the side of the tube.

834 NORTH NINETEENTH ST., PHILADELPHIA.

DISCUSSION ON ETHERIZATION BY THE RECTUM.

DR. O'HARA: I do not think I would use this method. I see no advantage in it over administration by the mouth. It involves more risks. The ether has to go through the portal circulation, and penetrate in that way

through the entire system. A good deal of local irritation will be produced, and the method might be followed by congestion of bowel. For operations above the mouth and throat it may possess advantages of convenience over the ordinary method, but a case of hare-lip operation has recently terminated fatally.

DR. LEVIE : I have had no experience in this method, but I have watched the progress of it. The main objection is the irritating quality of ether. Other anæsthetic vapors are not so irritating, and it might be well to try the action of some of these.

DR. NANCY : Although I have had no experience in the rectal method of inducing anæsthesia, yet I fully recognize that the ordinary methods of administering ether are unsatisfactory, and therefore I welcome the paper of the evening as a step in the right direction, i. e., the endeavor to discover some more satisfactory method of inducing anæsthesia. The rectal method, however, is infinitely less safe than by the mouth; one, if not more, deaths having been acknowledged in a few dozen cases—probably less than twenty-five patients in all having been experimented on—while about one death in 23,000 was the mortality usually given for etherization by the air-passages. The rectal method evidently requires much more skill and special training than the ordinary method. Anæsthesia I always considered a dangerous state, and I think that the usual custom in our American hospitals of entrusting the administration of anæsthetics to the junior member of the house staff, is a reprehensible practice. Instead of giving the anæsthetic into the hands of the least experienced resident, it should be intrusted only to the most experienced.

DR. DAVIS : In etherizing there are two things I am afraid of, suffocation and collapse. The former is usually readily avoided by attention to the tongue and the use of a gag to open the jaws; the latter is more serious—it occurs most readily in strumous children. In the University clinic there are constantly being performed operations on just such cases, resections and the like. In these strumous cases, particularly if the operation is a severe one, the depression is very marked. I have seen the temperature fall as low as 94°, recovery ensuing. If collapse threatens, the first thing to do is to withdraw the ether. If it is being administered by the mouth this can readily be done, but not so if it is being given by the rectum. In one of the cases related by the author, anæsthesia continued for a time after the withdrawal of the anæsthetic, and this is just what is to be feared in this method of giving ether. If symptoms of collapse supervene we cannot withdraw the ether from the bowel, and the anæsthesia must increase with a possibly fatal result.

DR. ALBERT H. SMITH : The cases offered by Dr. Miller are not sufficient to establish the advantage of this method. Perhaps in operations requiring but a few minutes, the method may answer, but how about cases in which the administration must be kept up for an hour or more. Anæsthesia is always to be considered a dangerous condition, but there is no special danger in a short anæsthesia if the material used is pure and carefully used. I

cannot see any advantage in the rectal method. There is a serious æsthetic objection to it. In operations about the mouth it may be convenient, but here we can use other anæsthetics. I do not think that there is any difference in the action of the ether in the two methods. The ether must always act through the nerve-centres. The difficulties of dyspnœa and irritation may be all avoided by the use of morphia hypodermically before the operation. In reference to the relative danger of ether and chloroform I may say that I have seen much more alarming symptoms from the former than from the latter. I have abstained from using chloroform in many cases, not because I thought it unsafe, but because I knew that if death occurred under its use, the anæsthetic would be made to bear the blame, while if ether were used it would not be charged with the accident.

DR. W. R. D. BLACKWOOD: To my mind the method is useless and objectionable. The mortality directly attributable to its employment is enough to prohibit the practice. I have no experience in the human subject yet, and would not have hereafter. I was asked to notice the subject in a medical journal, and made two experiments in order to learn something of the method. In one case the animal's abdomen was enormously distended, and the vapor *could not be removed* by simply affording free exit by a tube per anum, or by auxiliary external pressure; the vapor undoubtedly got beyond the ileo-cæcal valve, and was retained. We know nothing about the ability of the rectum to absorb gases; that is not its function. We cannot control retained vapor in the bowel; the procedure is dirty, offensive to all, and unjustifiable. Like all new things, too many in the profession will run wild over this plan for awhile, and then drop it for the last novelty, without regard for its utility or real worth.

DR. MILLER, in closing the discussion, said: The design of this paper, which I had supposed obvious, is the contribution of certain data to the subject of rectal etherization, and a formulation of the more obvious advantages and disadvantages. The attempt to strike a balance of the same I deem as yet premature. The mechanical dangers of overdistension, the difficulty of emptying the bowel of vapor, when a suspension of the anæsthesia is desired, and the greater caution needed in the administration—all these points I had already mentioned in my paper.

There are only two points to which I would further allude, viz. :—

First, as to rate of elimination: This taking place by the lungs, no matter how introduced, would be more rapid than when the agent is inhaled, inasmuch as by the new method the pulmonary mucous membrane is preserved intact, and therefore more capable of osmotic function than if bathed in mucus, as by the ordinary way.

A more serious objection has not yet been referred to—one based upon theoretical considerations. The experiments of Paul Bert—now already classic—have demonstrated :—

1. That the degree of anæsthesia depends, not upon the absolute amount of the agent used, but upon the percentage in the blood, and therefore on the tension of the vapor in the *atmosphere inhaled*.

2. That the percentages needed to suspend respectively the functions of animal and organic life bear a definite ratio to each other—a ratio constant for each of the known anæsthetic agents, and for each species of animal and for each human individual. All between the two percentages mentioned, is termed the *manageable zone*.

3. That most, if not all, the undesirable effects of an anæsthetic, are due to leaving this zone.

4. That the greatest safety is therefore in mixing the gases beforehand—as has long been done by Mr. Spencer Wells.

If now, ether be given by the rectum, it will be readily seen that the gauging can only be by absolute quantity, and not by the percentage actually in the blood. We could never know how near this zone is to being exhausted. To my mind this is the most serious objection that can be offered.

HOW CAN PHYSICIANS AID IN ELEVATING THE PROFESSION OF PHARMACY.

Read June 25, 1884.

BY F. E. STEWART, M. D., PH. G.

THE colleges of pharmacy, both at home and abroad, teach that pharmacy is a profession, and it is the wish of all true lovers of the pharmaceutic art, to elevate the practice of pharmacy to the dignity of a profession, equal in rights and privileges with the medical profession, with which it is closely associated in common interest. The physician and pharmacist are mutually interested in the study of drugs, the latter for the sake of supplying the demands of the former. In supplying the demands of a rational therapeutics, pharmacy must find her advancement both as a science and an art. In the study of drugs the pharmacist and physician should meet, and should consult as to the best way of preparing them. On the results of this study and consultation, a literature should be founded, which would constitute the joint contribution of the two professions to science. This science should represent the knowledge of drugs, their preparation and their application to the treatment of the sick; in other words, should comprise under one head *materia medica*, pharmacy and therapeutics. A name should be given to this science. It should be called pharmacology, or the science of drugs. These three branches have been classed under this general head in the past, and are now by several writers. One well-known writer defines

pharmacy as the science of preparing medicines, therapeutics as the science of applying medicine to the cure of disease, *materia medica* as the collection of substances employed in medicine, and pharmacology as the general term employed to embrace these three divisions. An able German writer says that "pharmacology in its widest scope embraces the study of drugs from all possible points of view, and the information thereby acquired may be useful under the most diverse conditions: to the physician, to enable the recognition and proper treatment of cases of poisoning, or to permit of the use of drugs for therapeutic purposes; to the public, to permit the avoidance of noxious substances; to the physiologist and pathologist, to enable the application of information derived from the study of the action of poisons to the advancement of their sciences."

In my little monograph published in 1882, and entitled "An Old System and a New Science," I have formulated this classification in the following language:

"Pharmacology in the science of drugs. It professes to teach what is already known or may be learned concerning drugs in the forms of exact observation, precise definition, fixed terminology, classified arrangement and rational explanation. This science, therefore, embraces in classified forms, *materia medica*, or the substances employed in medicine, pharmacy, or the preparation of medicine, and therapeutics, or the application of medicine to the cure of disease."

In other words, "the science of pharmacology includes knowledge of botany, agriculture, history, chemistry, microscopy, toxicology, pharmacy, physiology and therapeutics. A knowledge of botany is required to identify, and properly classify, medicinal plants; a knowledge of agriculture to understand their cultivation; a knowledge of history to compare them with each other with regard to their relative importance as remedial agents; a knowledge of chemistry to investigate their active principles; a knowledge of microscopy to determine their structure, and for the purpose of identifying drugs, and preventing adulteration and substitution; a knowledge of toxicology to determine their properties as poisons; a knowledge of pharmacy to prepare them aright; a knowledge of physiology to determine their physiological actions; and a knowledge of therapeutics to ascertain their therapeutic properties. This knowledge, classified into the forms

of science, and protected by a definite, changeless nomenclature; constitutes pharmacology or the science of drugs."*

To promote progress in this science, I believe that the practice of pharmacy should be elevated to the dignity of a liberal profession, like law, theology and medicine, and that the pharmacist and physician should unite in the study of drugs; that the pharmacist should practice pharmacy in the same manner as the physician practices therapeutics—to make a living; and that both should gain reputation and professional position by scientific work in their respective fields, and the contribution of the same to the literature of the profession. This I believe, in opposition to the commonly accepted belief that pharmacy is a trade.

A liberal profession is distinguished from what is called a trade by the ideal expressed by the word "liberal." Liberal, in this connection, means philanthropic—the service of mankind. The ethics of a liberal profession demand as the first thing a service to humanity, and especially a service to the profession. Anything that has a tendency to injure the cause of humanity, or to deprive the profession of benefit, is considered highly unprofessional. It is the duty of a professional man to publish his discoveries for the benefit of humanity and the profession. To keep the knowledge of a discovery secret for the purpose of making money, is considered a disgrace. A trade, on the contrary, is supposed to be run on no such liberal policy. What it discovers it appropriates for money-making purposes. But a trade cannot appeal for recognition as a liberal profession. But a distinction should be made, both in the case of the pharmacist and the physician, between the man who makes money his god and sacrifices the professional ideal on the altar of selfishness, and the unselfish professional man who finds his highest pleasure in the service of science, his profession, and the cause of humanity. The former should be rewarded in proportion to his service; the latter condemned.

We have in the field of pharmacy at the present time, the retail pharmacist and the manufacturer. The former, consists of the individual pharmacist controlling his own business, and the latter, comprises a collection of individuals, often a combination of pharmacists and business men, the business men controlling the

* This definition of what is necessary to constitute a science is drawn from Porter's Psychology.

policy of the house. It is apparent that the former should be held responsible as an individual, while the latter is responsible as a firm. Both must be cut and trimmed to fit the professional ideal, and at the same time the professional ideal must be modified to meet the requirements necessitated by dealing with these two factors in the pharmaceutical world. We have to consider, therefore, the ideal retail pharmacist and the ideal manufacturing house. Both should be animated with a professional and scientific spirit; both should practice pharmacy and make money by so doing; and both should gain position and reputation by the amount of work done for the benefit of science and their profession.

Possibly we can get at a better understanding of these two ideals, and the distinction that exists between them as applied to the retail druggist and manufacturing house, by considering the fields of science in which they should work. It will be conceded a wise public policy that limits the practice of pharmacy to the educated and licensed pharmacist, and protects him in his position by law. It will also be conceded that the educated pharmacist who practices his art in a scientific and professional spirit, has a right to be jealous of this protection. And the same thing applies to the practice of therapeutics. No physician will concede that any one has a right to practice therapeutics without thorough training in the knowledge of disease and its treatment, and he is jealous, and rightly so, of the right that he has gained by so much hard work and self-denial. The physician, therefore, resents any encroachment on his field of therapeutics, on the part of the pharmacist, and the pharmacist resents the existence of the manufacturing houses as an encroachment on his field by business men neither educated in drugs or their preparation. And a like argument is made with regard to the scientific field, and both professions say what right have the manufacturing houses to do scientific work. But the manufacturing houses exist; have, on the whole, done much for the advancement of pharmacy, and form a very important element in the solution of this problem. Furthermore, one of the manufacturing houses has as its head a physician of high professional standing and scientific reputation; another employs a dozen or more educated physicians and pharmacists, and most of them have in their employ one or more representatives of the medical and pharmaceutical professions. Why, then, should not these houses, if they take a professional and scientific

stand, be admitted to the ranks of scientific and professional men in their capacity as firms? * Why should they not do scientific work? Why should they not make money by the practice of pharmacy at the same time?

In formulating the ideal of the scientific and professional pharmacist, I can do no better than quote an answer to an editorial in the *Therapeutic Gazette* for March, 1884, entitled, "Shall we have a Profession of Pharmacy?" by a pharmacist of high reputation.

"But pharmacy to advance to the position of a profession must do something on the positive side. To become a science pharmacy must have a literature, as you have said; and while I hold that it has one now, this, however, is restricted to pharmacists alone, and at no time becomes such a portion of medical science as to receive the attention of medical men. Thus we have in our medical colleges no department worth considering, where pharmacy is taught or even demonstrated. The branch of *materia medica* and therapeutics deserves no longer the title of the former, for the knowledge of drugs, their character and preparation, has long since been left out of medical education; so much is the latter the case that the physician of to-day knows little of what he is prescribing, nor could he at any time distinguish good from bad, or judge by inspection or analysis of the quality of his medicines; scarcely, if at all, does he indeed know the strength of regular galenical preparations. The medical profession depends in this upon pharmacy, and yet are unwilling to give pharmacy a recognition in its proper place. In order to raise himself to a professional standard that may be accepted by the medical profession, the pharmacist must do scientific work and publish his results in medical literature. Each pharmacist should thus endeavor to contribute something to medical knowledge in his department of medical science, and by so doing he will receive the reward of professional position, and elevate himself beyond the tradesman and shop-keeper. The preparation of medicine must depend upon therapeutics, and the pharmacist who prepares medicine in the best manner to meet therapeutic demands, informing physicians of the fact through the medium of a carefully

* Not directly as firms, but indirectly through a scientific department of one or more professional and scientific men connected therewith, as explained further on; but the firms held accountable to science and the profession.

written scientific article concerning such a medicine, published where it will meet the medical eye, will surely receive credit, and create a demand for his goods thereby, for the physician will naturally believe that the pharmacist who knows the most about a preparation, is the one most capable of preparing it.

"To sum up, therefore, it seems to me that the elevation of pharmacy to the position of a profession depends upon the following requirements: First, the abrogation of the proprietary-medicine trade in all its forms; second, scientific work and literature upon the part of the pharmacist. To the latter it may be urged, however, that pharmacists have already done, and are doing, a great deal of scientific work which has appeared in pharmaceutical journals. This, no doubt, is true; but first of all, scientific work of that kind is not general amongst pharmacists, and is usually limited to teachers and students. The practical pharmacist usually loses sight of it in the pursuance of his trade vocation. Again, such work as has been done, has been of little service to medicine, buried as it is in pharmaceutical literature, not even extensively read by pharmacists, and totally ignored by the medical profession. I would, therefore, suggest that a distinction be made, and a final line of demarkation be drawn at this point, and that upon the one side of that line be placed those who practice pharmacy as a profession, and on the other those who practice it as a trade. Let the former purge their establishments of proprietary medicines entirely, and devote themselves to scientific work, and the cultivation of the practice of pharmacy. As the word pharmacology has been chosen to represent the science of drugs, let professional pharmacists be distinguished by the title pharmacologists, as suggested by Dr. F. E. Stewart. Let pharmacy be recognized as an essential branch of medicine, to be taught in its full extent in medical schools; let pharmaceutical literature be allowed a place with therapeutics in medical journals; and also let the scientific pharmacist be accorded an honorable place in medical discussions, many of which often smack of absurdities from the simple want of knowledge on this subject, so essential in the treatment of disease. The adoption of this course would be to raise up a profession of scientific men devoted to the practice of pharmacy as a profession, and, as they would have the interests of their science at heart, they would have a professional and scientific spirit. Professional pharmacists

bound by medical ethics would not be guilty of unprofessional conduct, so that the criticisms made on that score, referred to in your editorial, will not apply to them."

The manufacturing houses, however, employing as they do both physicians and pharmacists, have a much wider field for work than the retailer. It is from them, therefore, that we ought to expect the most rapid progress in the science of drugs. Each manufacturing house, of the sort I have described, should constitute itself into a laboratory of pharmacology, devoted to the promotion of progress in the knowledge of drugs, their preparation and application to disease. In other words, these houses should do scientific work on the *materia medica*, and make a living by the practice of pharmacy, and carry out the professional ideal.

To carry out this laudable scheme, the manufacturing houses should utterly repudiate the proprietary-medicine system in all its forms, drop all secrecy, and throw open their laboratories to professional inspection, publish their formulæ, and stand on the broad ground of scientific and professional pharmacy. They should in no way allow their scientific department to be subsidized by their commercial interests, but in all things should serve science first, commerce afterwards. They should practice pharmacy with the same liberal and beneficent spirit as the true physician practices therapeutics, and gain a living thereby; and should be rewarded by scientific reputation and professional position in proportion to their contributions to scientific literature. Such a policy would benefit science, the profession of pharmacy and medicine, and the cause of humanity.

I know that some argue that it is not permissible for a commercial house to do scientific work on the drugs or preparations in which they are interested, for the reason that scientific work should be done *pro bono publico*, and that such a system would be manipulated for commercial ends. They quote, in proof of this, the desire of quinine manufacturers to keep the duty on quinine, for the sake of protecting their business and affording a monopoly. In this, again, will be recognized the ideal of professional beneficence that I have already described, and the belief that wherever a commercial connection can be traced in any plan, beneficence will be sacrificed to selfishness, for the sake of making money. To this I would answer, there is a great deal of difference between

the commercial question of duty on quinine and the question of scientific work on drugs. It may be possible for great monetary interests to unite for the purpose of cornering valuable drugs by the tariff or by other means, but this is merely a commercial question and has no bearing that I can see on the other. There have been corners in opium, quinine, and other staple articles, but that is no reason why scientific work should not be done on them. Scientific work on a valuable drug only increases interest in it, and enhances its value, thus making it all the more desirable for other houses to compete in the market, thereby cheapening the product; benefiting commerce, by adding to the industries of the country; promoting progress in science, by fresh knowledge, giving us new and valuable therapeutic agents, and benefiting the cause of humanity by all these. I look at any house, who, animated by a professional and scientific spirit, throw wide open the doors of their laboratory, publish their formulæ, and devote themselves to the exploration of scientific fields for the benefit of science, so that they may furnish us with new and valuable products from nature's storehouse, as worthy the highest praise and commendation. And the facts bear me out, for I have taken occasion to study the matter carefully. Let me quote from a letter received in answer to one of mine, recently written, to a large manufacturing house, who, in addition to their manufacture of a full line of pharmaceutical preparations, are well known to the profession through their enterprise in the introduction of new drugs. They say :—

“It would appear that our relation to the introduction of new remedies is quite generally misunderstood by the medical profession. The impression prevails that the profit, from the sale of such drugs, is sufficient to tempt the introducer to make such misrepresentations touching their properties as will stimulate their sale. That you may correctly understand our position in this matter, we take the liberty of stating it, at some length, as follows :—

“New remedies are introduced by us to the medical profession in a sincere desire to improve the *materia medica*. The direct benefit, from a business point of view, which we hope to achieve is such recognition of our services to science and humanity as may attract attention to our regular business as manufacturers of legitimate pharmaceutical preparations. Such being our position,

we are, naturally, as a matter of sound business policy (if you will give us credit for no higher motive), anxious to obtain and publish, rather than to conceal, all possible knowledge with regard to the botany, chemistry, microscopy, physiology, and therapeutics of each particular drug which we introduce. Drugs are introduced under their botanical names, so far as our abilities, through the assistance of our scientific friends, will permit. We solicit, and by no means shun, the co-operation of scientists in physiological and therapeutical investigations. We publish favorable or unfavorable reports as they are submitted, and without mutilation or alteration. We do not solicit or publish testimonials as such, to the value of our preparation of a drug, but, as far as possible, secure articles written from a purely scientific standpoint covering the properties of the drug itself. Our endeavor is, by means of our systematized correspondence, capital and business enterprise, to afford to the profession facilities for testing agents hitherto not obtainable. In order to fix the value of such agents, we earnestly ask the co-operation of the medical profession; and only accurate and reliable information is asked, for it must be very apparent to any one who will give the matter a moment's consideration, that we can have no possible interest in introducing a drug which will not bear out in practice the claims made for it by its introducers. The sooner we learn the real value of a drug the better it is for us individually. If it has no such merit as may warrant its permanent addition to the *materia medica*, we are interested in discovering the fact early, so that our expense may be confined, as nearly as may be, to the original cost of obtaining our supply of it.

"It has been assumed, and is frequently so stated to us, that the introduction of new remedies is an exceedingly profitable branch of our business. We are willing to admit that it might be made such were we to control, in any way, the market, with the profession and public alike, for drugs which have been proved to be valuable over the existing agents in the *materia medica*. Such control we have, however, never desired or attempted, and, except in a few isolated cases, our activity in the introduction of new remedies has secured us no advantage in the market. From the very nature of the case those drugs best adapted to our purpose are such as have been hitherto unknown, and which have not entered into the ordinary channels of commerce. To secure such

we are necessarily compelled to send our own agents to the countries or districts where the drugs are indigenous for the purpose of collecting our own supplies. The next step must be to create a demand, and to this end we aim to publish all procurable testimony upon the subject, from legitimate sources, covering the botanical, chemical, physiological and therapeutic features and properties of the drug. We then isolate such active principles as it may contain, and make pharmaceutical preparations of the drug, according to recognized standards. These we distribute, free of cost, to such physiological experimenters and experimental therapeutists as will undertake to test them. Our great drawback in this branch of the work has been in the backwardness of the profession to co-operate with us. Having in this manner secured proper data for the guidance of the general practitioner to the further employment of the drug, we place at the command of the medical profession, its eleemosynary institutions, and its individual members, for experimental purposes, such simple fluid extract as best presents the properties of the drug. The expense involved in this donation and distribution of material is little known and less appreciated by the receivers, for they regard the amount only that is indicated in their individual instances. We are now ready to receive and publish all obtainable facts, of a legitimate nature, bearing on the value of the drug in practical therapeutics, and all information thus gathered is subsequently issued through the medium of our 'Working Bulletins.' This collection and publication of material involves further very considerable outlay of capital, which, owing to the inertia of the medical profession, and its tardiness to improve the opportunities afforded them, is necessarily idle for a lengthy period; sales of the drug are made slowly, and it is often years before we receive a return for our investment. Long before any possible profits are assured, the drug, by means of our effort and the enterprise of our competitors in appropriating the fruits thereof, becomes an ordinary article of commerce upon the market, where the price of the crude drug is much lower than its original cost to us. Pharmaceutical preparations of it are offered by competitors at less than our prices, and in some instances (such is one of the peculiarities of competition), the prices are cut so low as to remove all the margin of profit, even to those who manufacture from the cheaper supplies of the crude drug. The only protection that we

have in such cases is in the appreciation by the profession of the undoubted genuineness of the drug which we have to offer, and its recognition of the fact that our experience, and our study of the drug, have qualified us for making the best representative pharmaceutical preparation. Among the drugs which we have introduced, those which have borne us commensurate profitable return may be counted upon the fingers of one hand, and, even in the case of these profitable ventures, we have been burdened with expense, annoying effort and scientific labor, which are in their inherent nature very discouraging to business enterprise. The difficulties which are inseparable from this line of work find their compensation only in the benefit to our reputation as workers for the benefit of science and humanity, and the indirect effect of this enterprise upon our general business. We hold the personal ends which we have in view in this work are as legitimate, and in themselves as commendable, as are those which stimulate the more strictly professional worker.

"In the case of several new drugs which we have introduced, the cost of the crude material laid down in our laboratory, has been as high as from 75c. to \$7.00 a pound. Subsequent to the outlay which we incurred in our process of introduction, and the creation of a market therefor, as above stated, native drug-gatherers have placed them upon the American market at a tenth of its cost to us. *Eucalyptus globulus*, first brought before the medical profession of America by ourselves, cost us originally in the New York market \$1.75 in gold. It can now be purchased in the open market in quantity at from 7 to 10 cents per lb. *Jamaica dogwood* cost us through our own agency in collection, at least, 75 cents per lb. This drug has been offered upon the New York market at 6 cents per lb. It is true that the article so offered has been of inferior quality, and frequently the bark of the tree, instead of the bark of the root, is sold. Nevertheless, it finds a market, and we necessarily suffer in competition. The same may be said of *Manaca* (*Francisea uniflora*), and other drugs which we have not the room here to enumerate.

"Without canvassing this subject further, we would express our surprise that the medical profession do not more readily take advantage of our willingness to co-operate with them. Can it be that their backwardness is due to an impression that we are actuated by the same policy which induces the introduction and

sale of the so-called trade-mark pharmaceutical specialties—by which we mean a compound of drugs requiring no extraordinary ability as to conception or preparation, protected by trade-mark or patent, or introduced to the profession under concealed or misrepresented formulas? We have certain knowledge that such specialties can be and have been marketed to an enormous extent, and with great pecuniary profit. Comparatively little labor, advertising expense or business ability is involved in their manufacture and introduction. When the introduction and sale of a protected nostrum with assured profits, is contrasted with the labor and expense and unprofitable results involved in the introduction of an absolutely new remedy from the storehouse of nature, there must, it would seem to us, be a distinction made by scientific men, between the quality and nature of the two kinds of work.

“Our laboratory is open at all times to inspection by those seeking scientific or technical information for legitimate purposes. We publish working formulas and all possible information with regard to our products. We do not seek the aid of the government, in any form, for our protection, except in so far as our name itself as attached to such preparations, should be protected. We have published our platform broadcast in the medical press of this country, and we invite the strictest investigation of our methods by the medical profession, with whom it is our business, as pharmacists, to co-operate.”

But it must not be forgotten that pharmacy is a trade as well as a profession, and that certain commercial elements of the greatest importance enter into the discussion of this subject. The first element is that much-vexed question of supply and demand. The demand, which it is the business of the pharmacist to supply, comes from two sources—the medical profession and the public,—and it is an important question how far the pharmacist is to cater to the demand of his patrons. First, how far shall he cater to the demands of the medical profession? The demands of the medical profession upon the pharmacist can be seen by consulting his prescription-files. It will be found that the profession demand crude drugs, pharmaceutical preparations, such as fluid extracts, syrups, tinctures, etc., home-made and made by the large manufacturing houses; likewise proprietary pharmaceuticals, characterized by the now notorious *ine* and *ia* ter-

minations, such as lactopeptine, cosmoline, bromidia, etc., etc.; also proprietary medicines advertised to the people; and the so-called "patent" medicines, such as Ayer's Pills, Jayne's Expectorant, and the like. All of these things are prescribed by the medical profession, as any pharmacist will tell you. Shall the pharmacist keep everything that the doctor prescribes? If so, behold the list. Without answering this question, let me point out the effect of these various demands on the profession of pharmacy. As long as the demand of the medical profession is limited to crude drugs and the preparations of them made by the retail pharmacist, the latter is encouraged to select and prepare his own medicine; and if he does this with a professional and scientific spirit, and eschews everything unprofessional and unscientific, the professional ideal is satisfied, and professional pharmacy elevated thereby. But it is simply impossible for the retail pharmacist at large to obtain a living and carry out this pure ideal, for the simple reason that there is not enough demand for this kind of work for him to get a living out of it. Throwing aside the question of competition, which is a very important factor here, it is true that the medical profession have taken so largely to prescribing ready-made pharmaceuticals and special brands of well-known pharmaceutical preparations, such as ergot, for example, each physician having his favorite brand, that the selection and preparation of medicine are gradually being taken from the hands of the retail pharmacist, and he is becoming a mere dealer in ready-made goods. Of course, just to the extent that this becomes the case, the ideal of every pharmacist or professional man engaged in the selection, preparation and dispensing of medicines of his own manufacture, is lost. But it is a fact that the demand for ready-made compounds and special brands of manufacture are on the increase. Why is this? Is it a question of the survival of the fittest between the old system and the new, or is the medical profession so ignorant of pharmacy that it does not know the difference? Dropping for a moment the question of ready-made compounds, which are undoubtedly used by the physician for convenience in prescribing, let us consider the question of special brands of regular U. S. P. preparations. Should the specifying of special brands of well-known preparations be encouraged? I believe that it should, and I will give

my reasons in detail, for this is a very important subject, and one that is attracting no little attention at the present time.

It is our duty as physicians to use the best drugs, or preparations of them, that can be procured, and I hold that the pharmacist who can supply the best should be patronized. He should be encouraged in every legitimate way, and what more legitimate way than to specify his brand of manufacture, in preference to permitting prescriptions to be filled with inferior articles. By carefully studying this subject for the purpose of specifying the best brand of an article, a rivalry is engendered to see who will make the best. This is a most legitimate competition, and I claim that nothing would more quickly clean the market of frauds than to throw pharmacy open to legitimate competition of this kind, and for every physician to specify the brand which proves itself the best by careful examination of its composition, as set forth by the pharmacist, and clinical test of its merits. Nothing would have a more wholesome effect on professional and scientific pharmacy than this. Specify on your prescription, but do it justly, after careful study of the merits of the preparation. The demands of the physician, then, for different brands of articles in common use will be productive of much good. I know it is urged that the specifying of various brands necessitates the apothecary's carrying a large variety of brands of the same article. But this is not the affair of the physician. It is his duty, to himself and the public, to see to it that his patients get the best medicine the market affords; and the pharmacist has the remedy in his own hands, by improving on all the brands he has in stock, and convincing his medical patrons that his preparation is as good, or better.

But these remarks do not apply to proprietary pharmaceuticals. I have nothing but condemnation for the whole system. Nothing has done more to injure professional pharmacy than the demand created by the medical profession for them, which the pharmacist has been obliged to supply. Aside from the fact that before the law they stand in the same position as the out-and-out so-called "patent" medicines, and by sanctioning them by our prescriptions, we make the "patent" medicine business respectable, their proprietary and secret nature make them objectionable. They are objectionable from an ethical point of view, for the reason that every time we prescribe them we break a direct law of the

code, expressly forbidding it; they are objectionable from a scientific point of view, because it is only a question of time when the art of their manufacture will be lost for lack of publication, and their names no longer represent anything in existence, to the detriment of medical literature employing them. They are objectionable for the reason that competition in their manufacture is withheld from the pharmaceutical profession at large, the result being that the system affords no rivalry, to see who will produce the best brand, but offers a temptation to the manufacturer to put out an inferior article after the demand is once created, for the sake of making a greater profit in its sale. The demand for proprietary pharmaceuticals, therefore, on the part of the medical profession, is ruinous to professional and scientific pharmacy, and ought not to be encouraged.

And what has been said with regard to proprietary pharmaceuticals, applies with still greater force to the proprietary medicines, (the so-called "patent" medicines) advertised to the public. I am ashamed to own that it is necessary to enumerate this class of preparations in the demand created by the medical profession.

There is another and a varied demand created by the medical profession that should be taken into account, and a very important thing it is, especially to the large manufacturers and wholesale druggists. It is this demand for drugs and preparations of unlimited variety, caused by a difference of opinion upon the part of the medical profession, concerning the therapeutic value of such drugs and preparations. Take a large house, for example, whose market is the whole world. One country, or locality, employ a drug which is wholly unemployed, if not unknown in another country or locality. I hold that it is not the pharmacist's prerogative to be a judge between physicians concerning differences in therapeutic opinion, as he is not instructed in disease or its treatment. Here, certainly, he is forced to take the commercial stand and supply what is demanded, leaving the profession to decide on the merits and demerits of the article, from a therapeutic point of view.

I have suggested above, that each manufacturing house should constitute itself into a pharmaceutical laboratory for the study of the science of drugs, so that its products shall represent the results of scientific research in the various branches of that science. It is asked, how are you to harmonize the scientific and

professional ideal with the commercial ideal just described? In other words how is a manufacturing house, or a retail pharmacist, to supply his demands as a merchant, and satisfy his conscience as a professional man at the same time? I answer for both the retail pharmacist and the manufacturer: I think that any fair-minded man will admit that no one has the moral right to manufacture and sell an article detrimental to the interests of the public, or indorse a system injurious to the public welfare. If he believe the proprietary-medicine system a public evil, he certainly cannot manufacture and sell proprietary medicines conscientiously. It will also be admitted that a man who is endowed with a scientific and professional spirit, will not lend his encouragement to an unscientific system, or one that is injurious to his profession. The proprietary-medicine system is certainly unscientific, and entirely opposed to the professional ideal. It, therefore, follows that the retail pharmacist, or manufacturing house, making any claims to represent professional and scientific pharmacy, will not endorse the proprietary system in any of its forms, or take a course to injure either the profession of pharmacy, which it represents, or the profession of medicine, to which it caters, and of which it claims to be a friend. This is as far as the pharmaceutical profession can go. As I before said, questions of therapeutic differences must be settled by the physician, not the pharmacist.

To free the scientific ideals from their perplexing commercial and ethical complications, as much as possible, I have suggested the idea of a scientific department of experts, in connection with the large manufacturing houses who practice this art as above described. These houses can then be divided into two departments, commercial and scientific, the commercial department conducted in a manner in nowise incompatible with the true interests of science, but not claiming to be authoritative, and the scientific department devoting itself to scientific research, and gaining an authoritative position thereby. Following this classification, the literature of such houses could bear the stamp of the commercial or scientific department, according to its emanation, the former representing the general knowledge of the drugs and preparations handled by such a house, without regard to the source of the information concerning them, and the latter the authoritative statements of the scientific department. Questions

of therapeutics could then be considered by the therapist of the scientific department and the house relieved of the charge of teaching therapeutics.

Another very important point to be considered in connection with this question of supply and demand, is the introduction of new drugs. A physician in some distant part of the country writes an article concerning some new therapeutic agent, which he extols very highly in the treatment of some particular disease. This at once creates a demand for the article, which the various manufacturing houses, acting in their commercial capacity, seek to supply. Shall the commercial house be held responsible if the drug turns out to be a humbug? I answer, no. It is a question of therapeutics, for the medical profession, not the pharmacist, to decide. Here, however, the scientific department alluded to, could be of the greatest service to the profession. The commercial department, from the very nature of the case, cannot wait for extended scientific research. The demand is there and must be supplied. It is now the prerogative of the scientific department to take it up for investigation and report, and such a report may be received as authoritative.

A little study of this question of supply and demand will indicate to the physician how he can aid in elevating the profession of pharmacy. Indeed, much is in his hands, and he is, in great measure, responsible for the rise or fall of pharmacy as a profession, and when the importance of this branch of medical science is taken into consideration, how great the responsibility devolving upon him in this connection.

In this connection I would beg your attention to a few suggestions which I have made in relation to this subject of supply and demand, for the purpose of enabling the pharmacist, especially as represented by the large manufacturing houses, to supply the demand without injury to scientific and professional pharmacy, and also to enable the medical profession to co-operate, by their aid, for the purpose of elevating the profession of pharmacy. I first took up the proprietary pharmaceutical system to prove it unscientific, unethical and illegal. I have already pointed out its unethical and unscientific character. With regard to its legal status, it is only necessary to state that there is no law in this country granting an exclusive monopoly of an article of commerce but the patent law, and the patent law was designed to

prevent the very abuse so objectionable in the proprietary system. The patent law was devised to promote progress in science and the arts, not to retard their development by a system of secrecy and everlasting monopoly. This it aims to do by granting to inventors of new and useful articles the exclusive right to their manufacture and sale for a limited time, in exchange for a full knowledge of their nature and art of manufacture. This knowledge is stored in the patent office and is open for public inspection and study by the public at large, so that when any one wishes to improve such an article, he can study up the subject by means of the records of the patent office. By sending a small sum of money (25 cents, I think) to the patent office, any one can obtain full knowledge of the article patented. The patent system secures the publication of full knowledge of every invention patented, and thus benefits science; it affords a just protection to inventors until the investment of capital in working and perfecting the invention becomes a remunerative one and the inventor is rewarded for his labors, and the invention itself finally becomes common property, together with full knowledge of the art of its manufacture. Contrast this with the secrecy and perpetual monopoly of the so-called patent-medicine system. Contrast it again with the beneficent ideal of a liberal profession. Much may be said in favor of the correct application of the patent law to medicine, but I fail to find a single argument that will justify the proprietary-medicine abuse. It is, therefore, very essential to the progress of professional pharmacy that a clear line of demarkation should be drawn at the outset, between pharmacy and the proprietary-medicine system.

My next suggestion relates to the introduction of new drugs. As I have already described, a demand is created as the result of an article written by some physician describing its use in one or two cases occurring in his practice, and it is the business of the pharmacist to supply this demand at once, whether the drug turns out afterward to be of value or not, and it is not his province to set himself up as a judge of the therapeutic value of any drug. To permit the pharmacist to supply this demand, in a manner not injurious to science, and to secure the publication of full knowledge of the article, and protect the pharmacist by throwing the responsibility of its introduction where it belongs, viz., on the introducer, while at the same time furnishing a system whereby

both professions may unite in ascertaining the true value of the new claimant to a place in the *materia medica*, I suggested a collective investigation of each new introduction by means of the Working Bulletin system. This method may be carried out at the expense of the pharmacist or manufacturing house, by a medical or pharmaceutical society, at their own expense, by the government, or by a society organized for that purpose.

"This method of investigation consists of sending specimens of the drug to be investigated, either in the crude form or a preparation of the same, as the case may require, to a large number of practitioners scattered over the land, to the hospital service of the country at large, and to the various scientific centres connected with our leading medical and pharmaceutical colleges, with a sketch of the drug, stating the condition of existing knowledge concerning it, classified under the various heads of the pharmacology and known as a 'Working Bulletin.' The Bulletin is accompanied with a printed list of inquiries which those concerned are requested to answer from their observation, after having submitted the drug to careful tests. This information is then to be re-classified and published in the form of a report, which will be deposited, with a sample of the drug and its preparations, in the pharmacological department of the National Museum at Washington. It has been suggested that the National Museum, under the auspices of the Smithsonian Institution, be made a central repository for knowledge concerning drugs, so that any one wishing information concerning a medicinal agent may obtain it by applying there for it. This we consider a valuable suggestion, and take this means of contributing our quota toward this object.

"We do not claim that information collected in this way is conclusive, but that the method is a very valuable one for collecting evidence, and is a great help toward the final solution of the problem: What is the true value of the drug?

"The information of our final report will be classified as follows: 1st, Information from unscientific sources; 2d, Information from the profession at large; 3d, Information from hospital practice; 4th, Information from scientific experts engaged in more extensive research in the physiology, chemistry, pharmacy, etc., of drugs. The last class of information may probably be regarded as the more scientific, although each class has its comparative

value, and probably in the order of the above arrangement. Our first knowledge of nearly every medicinal plant in the pharmacopœia was obtained from Indian medicine-men, ignorant natives, quacks, and old women. Information from the profession at large, though not to be regarded as conclusive evidence, is of still greater value. Higher still in the scale are the results of hospital practice, for here greater opportunities are given for careful observation; but as has been pointed out by The Medical and Surgical Reporter (Dec., 1883, p. 635—'Methods of Investigation'), the observations of one logical mind, founded on extensive research, is probably more important than the 'collective unanimity' of the medical profession at large—though even such results have too often been set aside by more recent investigations, to be regarded as infallible. Until some method has been discovered more scientific than anything yet in vogue, we must depend upon information gleaned from all these varied sources, for our knowledge of the *materia medica*."

This system has already been adopted and put into practice by one of the largest manufacturing firms in the country, and I have provided myself with copies of their working bulletin on *Manaca*, a drug introduced from South America as a remedy for rheumatism, which I beg herewith to present for your consideration, as an illustration of the system.

In a lecture delivered before the Alumni Association of the Philadelphia College of Pharmacy, November 13, 1883, on the subject of "The Relation of Pharmacy to Therapeutics," I again brought to notice a suggestion made some two years ago, in an article published in the *Therapeutic Gazette*, at that time. The suggestion is a collective investigation of the *materia medica* of the world by the United States Government. To do this I would have founded at Washington a government laboratory, devoted to scientific works on pharmacology in all its branches, and the collective investigation carried out by means of the Working Bulletin system.

There is, at Washington, an institution known as the Smithsonian Institution. Its founding is due to that great lover of science, James Smithson. Its object is stated in the following clause of his will: "I bequeath the whole of my property to the United States of America, to found, at Washington, under the name of the Smithsonian Institution, an establishment for the

increase and diffusion of knowledge among men." The working of this grand institution is too well known to the profession for any description of it here. I suggested in the lecture referred to, that there be added to this institution, a laboratory of experimental pharmacology, for the purpose I have described. After discussing this lecture, it was voted as the sense of the meeting of the Alumni Association, that this suggestion be adopted, and that the founding of such a laboratory at Washington, in connection with the Smithsonian Institution, be recommended.

A collection of drugs, which, when completed, will represent the *materia medica* of the world, is now under way at the National Museum under the auspices of the Smithsonian Institution, and it has seemed to me that the addition of such a laboratory to this Institution, provided, as it is with peculiar facilities for scientific work, and beyond subsidy—being entirely outside of politics—might prove of great advantage. I accordingly sent a copy of my lecture, and a communication calling attention to it, to Prof. Baird, Secretary of the Smithsonian Institution, and received the following in reply: The first two letters are from Prof. Baird, the other is from Dr. J. M. Flint, U. S. N., Curator of the National Museum, to whom the matter was referred, and forwarded to me by the courtesy of Prof. Baird :

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SMITHSONIAN INSTITUTION,

Spencer F. Baird, Secretary,

Washington, D. C., Jan. 23, 1884.

Dear Sir :—I duly received your letter with the accompanying address, and am much obliged to you for calling attention to the National Museum as a suitable depository for collection of drugs and preparations.

We have received quite a number of interesting specimens from Messrs. Parke, Davis & Co., doubtless through your intervention. Every few days an offer comes from some wholesale dealer, among the latest being Messrs. Fritzsche Brothers, of New York.

We shall be obliged to you for any future effort on your part to increase the extent and importance of the Department of *Materia Medica* in the National Museum.

Respectfully yours,

SPENCER F. BAIRD.

TO DR. F. E. STEWART,

721 South 22d Street, Philadelphia.

(2)

REFERENCE BLANK.

SMITHSONIAN INSTITUTION,
Washington, March 8, 1884.
F. E. Stewart, M. D.,
Philadelphia, Pa.

The following is a copy of letter No. 32286, dated March 5, 1884, received from J. M. Flint, U. S. Nat. Museum. Please take notice of the same, and furnish me with information requisite for a reply, referring in your answer to the above number.

SPENCER F. BAIRD,
Secretary Smithsonian Institution.

(COPY.)

Professor S. F. Baird.

Sir—Concerning the letter of Dr. F. E. Stewart, herewith returned, I have only to say that the experimental study of the physical properties and physiological action of medicines, is of the highest interest and importance, and deserving of every encouragement. The creation of a department in the National Museum for such researches would involve the establishment of a chemical and physiological laboratory, and the assignment to it of experts skilled in such investigation. Of the desirability of such establishment there is no question; of its possibility under existing circumstances, you are the judge. I infer, however, that at present neither space nor funds are available for such a purpose.

I would respectfully recommend that the organization of such a department be favored, as far as may be consistent with the general plans of the Institution, and such opportunities of special study offered as the means at your disposal will allow.

Respectfully,
J. M. FLINT,
Curator.

The following letter was received at the time of reading the paper:—

SMITHSONIAN INSTITUTION,
Washington, D. C., June 23, 1884.

Dear Sir:—I duly received your letter of the 16th, in reference to the establishment of a pharmacological laboratory in connection with the Smithsonian Institution.

I wish it were in my power to take steps in regard to establishing such an agency, but at present we have no rooms available for the purpose, and no funds with which to sustain it. Perhaps in the course of a year or two, with the extended organization that we contemplate, something may be done respecting it.

DR. F. E. STEWART,
CARE OF PARKE, DAVIS & CO.,
Detroit, Mich.

Yours, Truly,
W. BAER.

I have now a final suggestion to make. It is the organization of a National Pharmacological Association, composed of those interested in the study of the science of drugs, in both the medical and pharmaceutical professions, who will unite in co-operating with the Smithsonian Institution in making this National Museum the great centre of pharmacological knowledge of this country. The importance of such a work no one will deny, and, if it is ever done, it must be accomplished by united action. In the study of the science of drugs both professions may unite, and in the earnest pursuit of this science, rivalry and bitter feeling will cease. Let us, then, have united action, organized, well-directed action, co-operative action, upon the part of all parties interested in furthering the increase of our knowledge of drugs; let us have a National Pharmacological Association and a scientific centre, untrammelled by trade or school affiliation, working impartially for the benefit of the whole.

Finally, I do not wish to be understood to teach in this paper that pharmacy and therapeutics should be practiced together. I have insisted so strenuously on the consolidation of the science of pharmacology, that there is danger that it may be inferred that I also advise the consolidation of the arts connected with this science into one art, which shall be called pharmacology as well. By a pharmacologist, I mean one educated in the knowledge of drugs, not necessarily skilled in the practice of all the arts that pertain thereto. Indeed, no one person could find time during the natural period of life apportioned to man, to perfect and practice all of these arts. It is sufficient, if he devote himself to any one of them in which he can make a living. But I do claim that the physician, to be a skilled therapist, should be thoroughly familiar with the nature and properties of drugs; and that the best pharmacist is he who, thoroughly instructed in their preparation, knows the most about their application as well. I also insist that pharmacy must always depend upon therapeutics, and that the closer the association of the pharmacist with the physician—the one who prepares drugs with the one who applies them to the treatment of the sick—the better it is for progress in pharmacy, and for the welfare of the physician and his patient.

The object of this paper is to gain the sense of the Philadelphia County Medical Society in regard to the establishment of a pharmacological laboratory at Washington, for the purpose

described, and to suggest the appointment of a committee, which shall be empowered to confer with the pharmaceutical profession and manufacturing houses for the purpose of drawing up a platform, containing regulations of such a nature, that pharmacists and manufacturing houses who observe them, may receive professional endorsement and fellowship, and be admitted to equal privileges with the medical profession. The adoption of this course will have a tendency to elevate pharmacy to the position of a liberal profession, and be of much better service to the profession, science, and humanity, than any attempt at legislation on a subject where there is so much popular misunderstanding, and where so much money will be used by the proprietary-medicine trade to prevent the enforcement of any law that may be passed.

DISCUSSION ON HOW PHYSICIANS CAN AID IN ELEVATING THE PROFESSION OF PHARMACY.

Reported by Wm. H. Morrison, M. D.

DR. H. C. WOOD: Dr. Stewart has used a large net with fine meshes in the preparation of this paper to which I have listened with unexpected interest, and has caught fish of all kinds, both good and bad, large and small.

In the first place, a very important point opened for discussion is the relation between the profession of pharmacy and the profession of medicine. Originally these two professions were united in one individual, the pharmacist and the doctor were the same person. I have always believed and I still believe that it has been a gain to both professions that the two have been separated. I do not think it possible at this day, when medical science embraces so much, for one man to know everything that pertains to it. In the two or three years of high-pressure work through which we squeeze our medical votaries into the profession, it is certainly impossible, and I think improper, to teach *materia medica* in the sense which a pharmacist has to know it. You cannot get a medical profession which will be composed of judges of whether this drug is or is not pure. In this world we must have faith in somebody, and in this case it is in the pharmacist. I do not think that we can gain anything by attempting to unite these two professions, which are distinct although correlated.

I think it were desirable, if practicable, to have some common ground, some common society, some mode of recognition, whereby the lambs of the pharmaceutical fold could be separated from the multitudinous goats which bleat everywhere. Whether this is practicable or not, I do not know.

There is another point in Dr. Stewart's paper in which we are brought on the verge of a battlefield of an internecine war which is waging between the manufacturing houses and the retail druggists. I must confess that I do not exactly see where this conflict is coming out, but I do see that we have no business as medical men to use or recognize any proprietary remedy, the various compounds whose names end in "ine," or any substance whose composition is unknown and various. Probably it is good at first, until a reputation is made, but then it deteriorates. Further than this I do not see that we can take any other action in regard to this struggle which is going on between the large and the small.

What seems to me the most important part of this paper of Dr. Stewart is that in reference to the formation of a national pharmacological association, in which both doctors and pharmacists may meet. I think that that is probably practicable as it is desirable. It would certainly be very desirable to have, in connection with the National Museum at Washington, a laboratory of original research in this regard. As is well known, the policy of this government is to maintain a speck of a regular army which shall be the skeleton of an organization that shall expand indefinitely in time of war. It seems a necessity of this plan of organization that there should always be in the employ of the government, numerous medical men for whom there is very little work, but who would be essential for the opening out of the executive minutia of hospitals in time of sudden war. Without expense to any one, some of these men could be well employed in connection with the National Museum in pharmacological research. It is well known that now at least one officer of the United States Army is employed in the National Museum to collect drugs and do the *materia medica* part of the work.

If we make a great ado about this matter, and attempt to found a great pharmacological laboratory, which, like Jonah's gourd, will arise in a day, it will wither when the sun comes out. What is wanted is to get the attention of two or three men of great mind and equal influence at Washington, and not attract the attention of the rabble which makes up the House of Representatives, and perhaps also the Senate, and having done this, allow this thing to grow until it cannot be set aside.

I think that it would be well for this Society to appoint a committee, not to plaster over and heal the wounds of our pharmaceutical brethren, but to determine whether or not it is at present practicable and proper to consider a movement for the establishment of a National Pharmacological Association, and especially to take measures for having the National Museum expand in the direction which we desire.

Perhaps I have wearied you, but allow me to say one word in regard to the relation of manufacturing firms to scientific investigation. If a commercial firm desires to set aside a certain amount of money for scientific investigation, why science gains. It makes little difference to science where the money comes from, provided that the scientific workers are not associated with anything that is disreputable.

DR. CARL SEILER : There are several points in the paper of Dr. Stewart

which struck me rather forcibly, and I am afraid that I must class them among the bad fish. In the first place, he claims that a druggist should be a pharmacologist according to his definition of that term, that is he should know all about the application of drugs, their action and so on. According to that definition, all that a physician would have to do would be to send the druggist a diagnosis and tell him to prescribe. For instance, a man comes with the clap, all the physician need do is to tell the druggist, "This man has the clap; give him a mixture." Another objection to that same point is that it would make the retail druggist a nuisance to the physician and to the patient. I have in mind now a druggist who was a nuisance both to the doctor and to the patient. A friend of mine had a case of pulmonary hemorrhage, and wrote a prescription for ergot. A sister-in-law of the patient took the prescription to the druggist, who, after reading it, officiously said, "I guess you have been having a baby at your house?" The consequence was that the woman gave him a piece of her mind, and the physician sends his prescriptions to another druggist.

There is another point; if the retail dealer is to be a professional man according to the doctor's definition, he is going to starve to death, for the simple reason, as is well known, that the prescription trade does not pay. It is only the fancy trade that keeps the pharmacist alive. If he is to be a professional man, he will starve. If he barely makes a living, he cannot afford to keep pure drugs for the physician to prescribe.

In regard to proprietary medicines, I think that we are fully justified in prescribing anything, whatever be its name, if we find that it is good for that particular condition. When I find that this, that, or the other preparation is good in such and such cases, I think that I am at liberty to prescribe it, whether it is a proprietary medicine or not. As a matter of course I always prescribe if I know the composition as to the ingredients. I may not know the number of grains of each, and if I did would not attempt to remember them. For instance, Platt's chlorides: I do not know its composition in grains, yet I use it. Again, I find listerine one of the most admirable preparations for certain purposes. I know that it contains thymol, this, that, and the other, but I do not know how much of each. It saves trouble in writing a prescription; I prescribe it; it may be a proprietary remedy, but it does good anyhow.

Another point: as Dr. Wood has said, it makes little difference who goes to work to investigate the physiological action of a drug, but it makes a difference how it is done. For instance, if a large manufacturing house sends a man to China, who has managed to get hold of a large amount of some root, and this large house goes to work to investigate the active principle of the root, and proclaims it to be a good remedy for so and so. For what reason? Because it wants to get rid of the large amount of root which it has, and wants to realize on the capital. If this is done, it is a bad plan to allow the manufacturing house to determine what is good and what is bad. If we could be sure that the investigation was carried on in a bona-fide way by competent men, by men who are not to be bought, or as suggested by Dr. Stewart, in a National Pharmacological Laboratory at

Washington, it would be a good plan. Let the specimens be sent there, let the experiments be made there, and when the results are proclaimed, we shall be satisfied, but we shall not be satisfied to take a remedy simply on the recommendation of one manufacturing house, who says that Drs. Smith, Jones, and so on—of whom we have never heard—have found it useful in such and such cases. This is not enough, to my mind.

MR. L. E. SAYRE : I have read Dr. Stewart's paper, and as I am not gifted as an extemporaneous speaker, I thought that I had better formulate my ideas on paper, which, if you will permit me, I shall be glad to read.

The relation of therapeutics to pharmacy is so ingrained that an advance in the former must necessarily call forth renewed effort in the latter. As the acquirements of the former increase, the greater the demand upon the latter. Pharmacy must progress in like ratio to that of therapeutics.

There was a time when both branches of medicine were, comparatively speaking, in their infancy. Since that time both have progressed toward a greater degree of perfection; and this progress has been practically evolved from mutual co-operation. We may not have been conscious of this fact, but I think close introspection will reveal that every marked improvement in the preparation of remedial agents has grown out of either the demands of the physician, on the one hand, or suggestions from the pharmacist, on the other. The various sources from which curatives might be derived, have been searched exhaustively, and in this connection we turn with pride to names in both professions familiar to us all.

Another landmark in the growth and progress of our profession is the increasing number of pharmaceutical associations, both city, county, and state, and, finally, the national association, combining their working in harmony. Before these associations, matters of all kinds are brought and thoroughly ventilated. Questions of the highest importance to therapeutics as well as pharmacy are discussed, the discussions filed, and eventually sown broadcast through the medium of pharmaceutical literature.

Our literature is another point worthy of note, collecting, as it does, the views and experience of the best talent in the profession, and distributing it to the public directly interested.

In summing up, therefore, I think I can safely say that the profession of pharmacy is in no way behind that of therapeutics.

Dr. Stewart says "pharmacology is the science of drugs." Is it advisable or practicable to organize a school of pharmacology? This is no new name. One of my earliest recollections of pharmaceutical literature was a work known as Parrish's Pharmacology, embracing all the points referred to in Dr. Stewart's classification, and whether an old name for a new science will add impetus to the movement is a question. All the branches enumerated in the doctor's classification are already embraced in the pharmaceutical curriculum, excepting, of course, that of therapeutics; and that is such a wide field that one might devote a lifetime to it and yet leave it comparatively unexplored.

Again, the tendency of the times seems to be toward specialism, and we have the various specialists, supporting their votaries with handsome

incomes, and the therapist doing justice to his special branch, finds in it a business sufficient in itself.

But our friend wishes to inaugurate a profession of to-day, which he says is progressive, by bringing in, again, those branches, which, by the inevitable law of differentiation, evolved a separation which is as distinct as dentistry and therapeutics.

Yet, to do the doctor justice, he says, after formulating his theory, that he does not wish to be understood as teaching that pharmacy and therapeutics should be practiced together. If this is really so, then we have wasted our time and energy in pursuit of a mere phantasy. If we leave therapeutics out of pharmacology, then the matter rests itself where it was originally, for, as we know, all the points raised by the new science are included in pharmacy, save therapeutics alone.

Eliminating, therefore, this question from the argument brings us down to the next suggestion, viz., that of drawing up a platform containing regulations of such a nature that pharmacists and manufacturers who observe them, may receive professional endorsement and fellowship, and be admitted to equal privileges with the medical profession. It seems to me that this proposition is exceedingly objectionable; and for the following reasons:

Dr. Stewart says "we have in the field of pharmacy at the present time the retail pharmacist and the manufacturer. The former consisting of the individual pharmacist controlling his own business, and the latter comprises a collection of individuals, often a combination of pharmacists and business men, *the business men controlling the policy of the house.*" If reference be made to a lecture delivered recently by Dr. Stewart before the Alumni Association of the Philadelphia College of Pharmacy, it will be found that he states that the pharmaceutical profession have been forced to turn elsewhere for support, in consequence of the introduction by manufacturing houses of what he terms trade-mark pharmaceuticals—as much protected as patent medicines. Seemingly elegant compounds of old and well-known drugs, but exact knowledge of them, whereby any one can manufacture the same, is never published, but is a thing of trade secrecy. Scientific pharmacists, he says, must unite against this abuse. "If therapeutics is to have a literature, the medical profession must join the pharmacists against the proprietary-medicine system in all its forms." •

I would say to the doctor: By your own words you have rendered nugatory your first proposition on pharmacology. By your own words you have placed individual pharmacists upon an antagonistic plane. By your own words you have swept away all useless verbiage and sophistry, and laid bare, what seems to most pharmacists, the real object of your proposition—not the elevation of pharmacy, but the elevation of the manufacturing houses. Some manufacturing houses do, indeed, employ physicians and skilled pharmacists, but what for? To elevate pharmacy? No, rather to give seeming substance to their scientific claims. How skilled those physicians may be, no one knows—at least not the public. How unscrupulously their, perhaps, really skilled pharmacists may make use of their knowledge, is

equally unknown. Those who hire them are no more entitled to be classed as scientists than the man who hires an orchestra is to be called a musician. Will Dr. Stewart tell us how many manufacturers there are, who, for the love of science, will risk their capital? Real scientists are "few and far between," and will not risk their reputation, nor hamper their movements, by yielding to the demands of any commercial company, which, of course, controls those whom it employs.

Let no one draw from what I have said that I have any personal feeling against any manufacturer: on the contrary, I admire the energy, tact, business principles and management which have placed some of them on a high standing commercially. I am looking at them from another standpoint entirely. From a business point of view I know that they are as reliable to deal with as any other class of men. I disagree entirely with Dr. Stewart, however, that the elevation of pharmacy needs the aid of his ideal pharmacological school and national laboratory, nor the intimate union of therapeutics with pharmacy. There are other ways which would yield more immediate and satisfactory returns than the plan he has suggested.

Finally, I will say, that physicians will ever find all true pharmacists ready and willing to enter upon any plan of intelligent action that will tend to fraternize the two professions, but we have the ideal of a pharmaceutical profession too much at heart to co-operate with those who are not professional pharmacists, believing that by this line of action we lower the standard, instead of elevating it.

DR. DWIGHT: It is with pleasure that I have listened to the discussion of the paper which has been read this evening. The remarks of the last speaker are very timely. This whole difficulty comes up very naturally in a Society which for a year has been discussing the difference between he and she, and has shown its inability to settle that vexed question.

The word "pharmacology" is derived from a Greek word which includes poisons as well as drugs, hence the word pharmacology means the doctrine of poisons as well as of drugs. When the meaning of that word is settled, the old word and the nomenclature connected with it will once more come into the department of *materia medica*.

We have at the present time such men as Pepper and DaCosta, abundantly qualified for the position, who have devoted their attention especially to this department of therapeutics. When we can set aside the experience of generations, we shall go back to terms which have been to a great extent forgotten, and simply introduce them into use. The attempt to confound therapeutics with pharmacy, will be a failure. I have no objection to men uniting these, but the attempt to make this Society a party to the effort would rather lead us into unknown paths, and place us in a position where we might lose what little character we possess.

DR. FRANK WOODBURY: I have taken considerable interest in the discussion. It is evident that it is one in which the lecturer has taken considerable interest, and that he has devoted considerable time to its study, and I think that we are under some obligations to him for bringing it before

us. I cannot altogether agree with him in the opening sentence, or even in the title of his paper. The title is: "How can Physicians aid in elevating the Profession of Pharmacy?" The opening sentence is: "The colleges of pharmacy, both at home and abroad, teach that pharmacy is a profession, and it is the wish of all true lovers of the pharmaceutical art to elevate the practice of pharmacy to the dignity of a profession, equal in rights and privileges with the medical profession, with which it is closely associated in common interest." I think that this would be an unfortunate admission to make. It certainly is an unfortunate doctrine to inculcate, that the practice of pharmacy is in any way a distinct profession from the practice of medicine; that they are in any sense to be compared or contrasted with each other. I do not think they should be arrayed against each other, or associated together on equal grounds. I think that pharmacy, if examined at all closely, will be found to be merely an outgrowth of medicine—a specialty, if you choose, to the same extent as the other specialties, as dentistry. It is unfortunate to admit that it is anything like a distinct profession from medicine, or that in any way pharmacists and physicians can be arrayed against each other, for their interests are identical.

I find a number of distinct propositions submitted for discussion. One of the most prominent ones is: Shall manufacturing houses do scientific work? We have no objections to their doing scientific work. Any one who is willing to pay for the work, is an acquisition to science if the work is properly done.

Another question which is raised, is our attitude to new remedies. Just at present the attitude of the profession to new remedies is one of forbearance. Just now we are suffering under an avalanche of new remedies, like the extract of that island in the Pacific Ocean, and then there are others which do not correspond with anything in the heavens above, or in the earth beneath, or in the waters under the earth.

What shall be our attitude toward proprietary medicines? Our attitude toward proprietary articles has been defined time and time again. It is not long since the County Medical Society had this subject before it. Its position is that it is unethical to use remedies of which we do not know the composition and which we cannot control.

With regard to the proposition for a national organization of physicians and druggists, it seems to me that there is a good nucleus for such an organization existing in the committee for the revision of the national pharmacopœia. I am not aware that it was the fault of the physicians that they were not continued on the combined committee. I think that that should be the commencement of such an organization and I should advocate the addition of physicians and pharmacists to that committee, in order to make a national pharmacological association.

With regard to a National Pharmacological Laboratory, at Washington, the idea is a good one for utilizing some of the idle talent which is at the service of the government in Washington, and it might be made useful to science. This seems to me to be the only practical proposition which has been submitted, and perhaps it is really the point which Dr. Stewart wished

us to discuss. Such a laboratory would very properly come under the care of the National Board of Health. With it might be combined a board of public analysis, which might furnish us with the composition of the various proprietary and patent medicines which are supplied to the people.

PROF. REMINGTON : I have listened with a great deal of interest to this paper. I am sorry that it has taken such a wide range. I am sorry that Dr. Stewart had so many interesting subjects. It seems to me that only the larger sized fish should claim attention. I was very glad to hear the remarks of Dr. Woodbury on one point for which I felt considerable anxiety. One of the previous speakers advocated the very radical doctrine that it was proper to prescribe anything that would do good under any circumstances. Such free trade in medicine would, I think, destroy entirely all scientific work. When I heard Dr. Woodbury state that the Philadelphia County Medical Society had taken official action, and decided that it was unethical to prescribe anything but medicines of known composition, I felt greatly relieved. I certainly hope that the medical profession will never take that position, for it is one which is exceedingly annoying to us, placing the pharmacist continually in an embarrassing position. On the one hand there is one set of physicians continually declaiming against the practice of encouraging the sale of patent medicines, and on the other hand there is a set of physicians who believe in free trade, and many go so far as to take the ground that they would not keep patent medicines if it were not that physicians frequently prescribe them.

With regard to the troubles between the manufacturing pharmacists and the retail druggists, I think that they can take care of themselves. I think that the numerical strength of the retail pharmacists is sufficient to keep down the commercial strength of the manufacturing pharmacists. It is a war in which physicians can afford to stand aside and let them fight it out. The result will, I think, be the elevation of pharmacy.

Dr. Woodbury touched on a point which I think is the key-note of the whole discussion, that is the formation of the National Pharmacological Association from a committee of physicians and pharmacists who have charge of the revision of the national pharmacopœia. There is a definite aim. If a pharmacological association could be formed in which physicians and pharmacists could meet on a common ground, and in which there was a provision in the constitution or by-laws shutting out entirely any discussion about business or trade interest, and allowing simply the discussion of scientific subjects, I think that we should find, that through the influence of such a society, we should have in 1890 a pharmacopœia which would not be the subject of so much adverse criticism as the pharmacopœia of 1880.

I had the pleasure of being at the Washington convention at the time of the appointment of the final committee for the revision of the pharmacopœia. I would say that the committee is continued, that the physicians and pharmacists are still working together, and that in a few years, perhaps next year, a supplement to the pharmacopœia will be issued, which will embody some of the recent work of the combined committee of physicians and

pharmacists. It was my first experience on a pharmacopœia committee, and I was never more struck with the need that there was for some organization in which physicians and pharmacists could meet on a common ground. You will agree with me, Mr. Chairman, when I tell you that a physician, I shall not mention his name, but a man universally looked up to, gravely proposed at that time that vaseline and cosmoline be introduced into the pharmacopœia, "because they make such excellent bases for suppositories." That man, in the diagnosis of a case, or in practical therapeutics, has no equal. So when the matter came up for discussion in the Washington convention, although the physicians were numerically the stronger, yet when the committee on the final revision of the pharmacopœia was appointed, it stood fourteen pharmacists to eleven physicians. This is the reason why the work of the pharmacist has been so plain. There was a preponderance of pharmacists on that committee. If we could get such a society—if there is some common ground on which physicians can come together and work side by side—it would be desirable. Whilst the profession of pharmacy is a specialty, we may say, I will not accept the position that it is not a separate profession. It is a separate profession, and, unfortunately, it has become in a certain measure antagonistic to the medical profession, simply because there has been no common ground on which physicians and pharmacists could meet and become acquainted. If such a society were organized, there would grow up what might be called middle men, who would be both physicians and pharmacists, who would serve to bring together the two professions, and supply that lack which was apparent in the work of the pharmacopœia committee.

I was present in this room, five or six years ago, when the County Medical Society considered some revolutionary propositions of Dr. Squibb, regarding the method in which the pharmacopœia should be revised. One of the censors of this Society got up and asked for information about the pharmacopœia, for he said that he had never seen one. I treasured that up in my mind, and when I went to Washington it seemed from the discussion as though nine-tenths of the physicians there had never seen the pharmacopœia. In regard to the pharmacopœia of 1880, I can only say, "Gentlemen, you have nobody but yourselves to blame. You had the majority of representation at Washington, yet you permitted the pharmacopœia to drift away from you. You did not come to do any work. Only one county medical society was represented, and that, I am happy to say, was the Philadelphia. No State society was represented."

The kernel of Dr. Stewart's paper seems to be the formation of some society in which physicians and pharmacists can be brought together upon some common ground. If such a society can be formed, the pharmacopœia of 1890 will show a great improvement over that of 1880.

DR. WISE: I think I can match Prof. Remington's story about the suppositories. I was attending a prominent pharmacist two or three years ago, and ordered suppositories. I afterwards learned that he had swallowed them, instead of administering them in the ordinary way.

The relation between the manufacturing houses and the retail druggists has been touched upon in the discussion. This concerns physicians as how they shall get the most efficient remedies. I find that many pharmacists do not make their own preparations, and therefore they cannot guarantee them. They buy them of the wholesale houses and the manufacturing pharmacists. They buy from these houses who ought to have clerks from the College of Pharmacy, but whose clerks are not so qualified. Some time ago, I had occasion to prescribe fluid extract of jaborandi. Instead of the specimen having the characteristics of a fluid extract, it had all the appearance of a tincture, which subsequent examination showed it to be. It is hardly necessary to say that it had no effect. Again, I ordered ten powders of one-twelfth of a grain of Clutterbuck's elaterium. The patient took the powders without having any effect produced. I think that it would be impossible in a certain part of the city, except in two drug-stores, to obtain a fluid extract of jaborandi or Clutterbuck's elaterium.

The last pharmacopœia recognizes the preparation of wines, as the wine of ergot, from the fluid extract. To make a fluid extract requires more skill than is possessed by most pharmaceutical clerks. When the wine is made from a fluid extract several sources of error are brought in. The powder may have been kept too long and be inert, the fluid extract may be made improperly, and therefore the wine is very apt to be inefficient. It is a common practice, in stores not very conscientious, to use fluid extracts as the basis for tinctures and wines, and the main care of the preparations is placed in the hands of an apprentice.

It has seemed to me that the additional fineness of the preparation as required by the last pharmacopœia, and the additional time and care necessary, has been one reason why the business has passed into the hands of large manufacturers. For instance, it requires the use of both methods of percolation and maceration, instead of the old plan of maceration, in which the powder is put into a bottle and the liquid added, and the bottle set aside for two or three weeks, being occasionally shaken, or, if a diaphragm is used, this is not necessary. This was the method used for many years, and is still used in Germany, and is allowed by the French codex. The increased amount of work put on the pharmacist has thrown the business into the hands of the manufacturing pharmacists, some of whom are good, but the most of them are not. I do not believe that preparations can be made as well on a large scale as on a small one. In some of these laboratories percolators fifteen feet in diameter and fifteen feet high are used, and I do not think the active principles can be extracted as thoroughly by a large percolator as by a small funnel.

PROF. REMINGTON: Mr. Chairman, I do not arise to speak to the subject, but as a pharmacist I cannot, unless you decide me out of order, permit some of the statements of the gentleman who has just spoken to pass unchallenged. In the first place, I think that it will be found that wine of ergot is not made in the way he states. In making fluid extracts, maceration cannot be used, for from 100 parts of the drug 100 parts of the liquid are obtained; if the liquid is added to the powder, a thick mixture of about

the consistence of mush would be obtained. It is therefore necessary to percolate. Fluid extracts are not known in Germany.

In regard to percolating in large quantities, the larger the operation, the more successful is the result, for it is not influenced by such minute changes. If 500 pounds are percolated at one time, an ordinary workman can obtain excellent results; while if only five grains, five drachms or five ounces are used, much more skill is required. I speak from knowledge, having had experience both in the large and in the small way. I think, however, that retail pharmacists should make their own preparations, for they should be held responsible for their articles. Many druggists employ the products of the manufacturer which often vary in strength.

DR. HENRY LEFFMANN: If the pharmacists and physicians think that the progress of the two professions would be benefited by an association, the only way would be for them to come together and form an association. It is not necessary for this Society to take any action. Let those in favor of such a society form it, as the physicians and lawyers have recently come together and formed a thriving Society.

DR. STEWART: I am much obliged to the Society for its courteous attention to the paper, and also for the discussion of it. I do not think that I can add any other points or anything further of interest, and therefore I shall be short and to the point and say no more.

ADDENDUM TO DISCUSSION ON THE TUBERCLE BACILLUS.

[Dr. Formad, who did not hand to the reporter in due time, the notes of his closing remarks in the discussion on tuberculosis, was permitted to furnish them subsequently for incorporation into the Proceedings.]

It is very gratifying to me that my communication on tuberculosis developed such interesting and profitable discussions in the several meetings of the Society. The expressions of opinion and detail of experience by the members were highly instructive to me, and a wholesome stimulus for further studies.

I desire to express my thanks to all the gentlemen who took part in the debate, and I was glad to learn that the weight of authority and experience leaned against a parasitic origin and the contagiousness of phthisis. I fully recognize, however, the authority of the two or three gentlemen who favored Koch's theory.

I desire in particular to acknowledge my full appreciation of the elaborate studies of Dr. Webb, who, independent and long before the outbreak of the "bacillus scare," and not merely in consequence of the latter, like the majority of our scientists, entertained the view that phthisis is contagious.

I must, however, take exception to a statement of Dr. Webb in regard to the view of Dr. Hiram Corson upon the subject. Dr. Corson himself has lately sent a letter to this Society, in which he expresses his firm belief in the non-contagiousness of phthisis.

I feel also thankful to Dr. Shakespeare for his unconditional opposition. Opposition is always useful.

In relation to the bulk of Dr. Shakespeare's statements, it is not necessary to say anything, as they were not pertinent to the subject at issue, and merely of local and biographical interest; moreover, they represent only personal views, more or less gallant in expression, and really concern no one, and hurt no one.

There is, however, in the report of Dr. Shakespeare's remarks, a statement of importance, and of such character that I cannot leave it unchallenged. I am quoted by Dr. Shakespeare as having made the declaration that "Koch had so far modified his views that he now admitted that neither the form, size, and aspect of the tubercle bacillus, nor its want of individual motion, nor its peculiar behavior toward staining fluids, distinguished it from any other bacilli."

This would be, evidently, a misrepresentation of Koch's view upon this point, and I am *not* guilty of it. What I ever said or published on this particular point was this: "Dr. Koch kindly demonstrated to me a number of specimens of bacilli, and, in particular, the appearance of these bacteria exhibiting under low amplification the peculiar S-like figure in the growths in masses. Koch seems now to lay more stress upon this low-power appearance and upon the pathogenetic properties of the *bacillus tuberculosis* as a distinguishing feature from other bacilli than upon the color test. During the conversation he admitted that some other bacilli may also yield the same micro-chemical reaction as the tubercle bacilli, but insisted that the latter bacilli cannot be stained brown. The failure of the tubercle bacilli to take the brown stain, he said, was the reason that they cannot be well photographed (blue- and red-stained objects not being suitable for photographing)." (See bacillus chapter in my second communication on tuberculosis.)

It is also cruel of my friend Shakespeare to say, as he does, in another part of the same remarks, *that I have accused Koch of unscientific work!* Those who have honored me, on various occasions, with their attendance in scientific meetings, in the lecture-room, or daily in the laboratory, and those who have read my papers, can testify that I have ever done justice to the mycological work of the discoverer of the tubercle bacillus, even if, so far, I have had no reason to agree to all his conclusions.

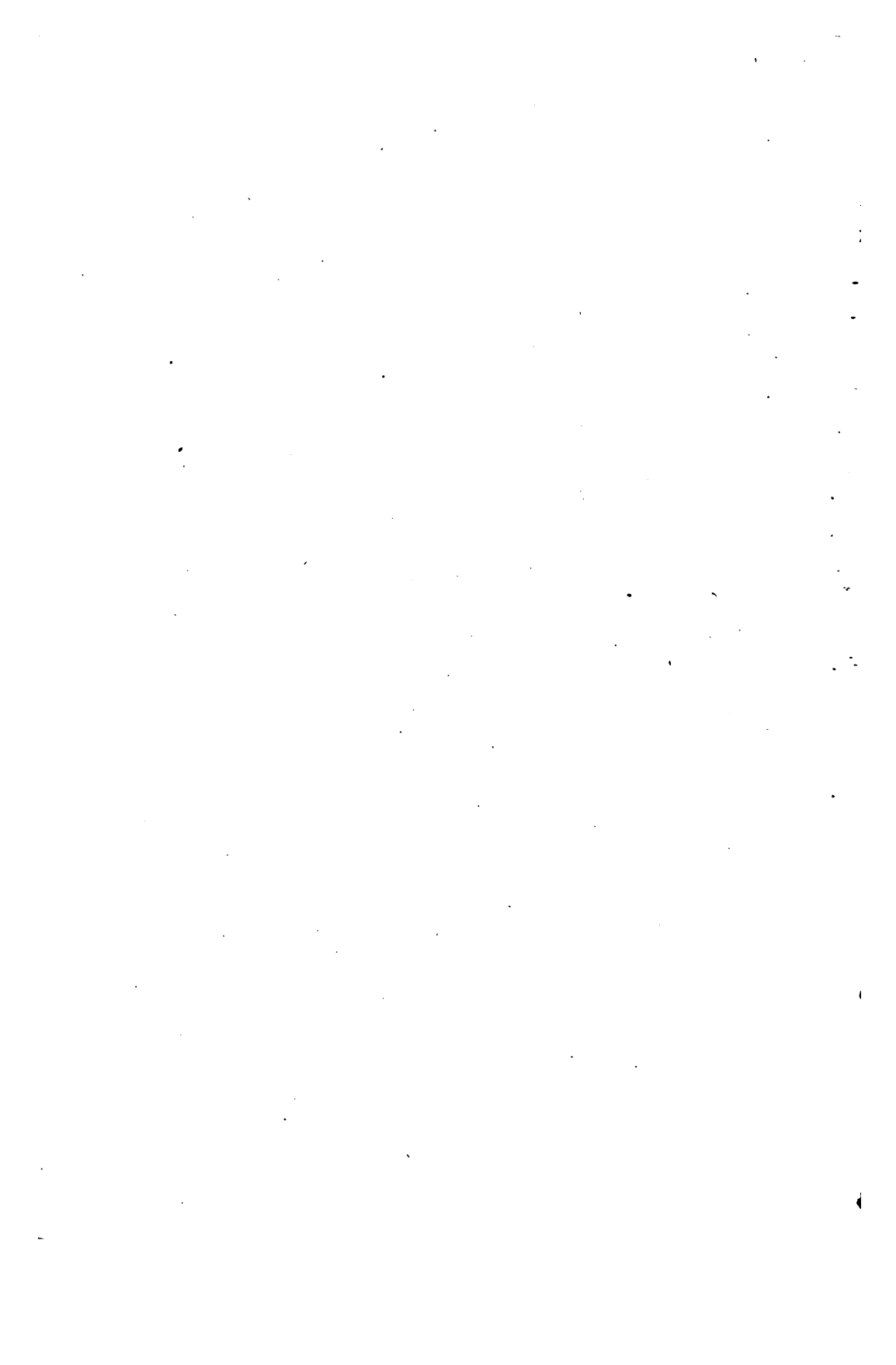
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